

# Antiquity

---

Vol. XVII No. 66

JUNE 1943

---

## Field-names

by F. T. WAINWRIGHT

**F**IELD-NAMES, the common and convenient term for minor names which have not achieved the status of place-names, often preserve information important to the historian and to the archaeologist. Their value is well recognized by specialists but not always clearly understood by others. Since field-names are generally found in local, as distinct from national, records, local historians are especially qualified to help in the accumulation of a collection of such names. After a few sentences of advice to secure uniformity of method and to ensure co-ordination, the members of any local historical society, acting as a group or even singly, may build up a valuable collection of field-names. Such work, pursued on systematic lines, would be a real contribution to scholarship, and is already in progress in one or two areas. The following notes attempt to illustrate how the study of field-names may contribute to historical knowledge. They do no more than outline the possibilities and they are confessedly written in the hope that more non-professional Societies will add the formation of a field-name collection to the list of their activities.

Place-names and field-names are of course fundamentally the same in origin: the primary object in each case is that of identification, to enable the surrounding settlers to distinguish in their speech the various villages, and, likewise, the woods, fields, trees and streams. In fact, many modern village-names are in form inseparable from minor names and it is probable that they arose as such. For example, place-names with non-habitative terminals (*-ford*, *-dun* etc.) must have arisen as minor names describing fords, hills etc., and not the villages and towns to which they later became attached. Yet, while remembering the essential inseparability of place-names and field-names, it is usually convenient to treat them as two classes of place-nomenclature. This artificial division is made desirable not by the nature of these names so much as by the manner of their occurrence. For each recorded village-name there are hundreds of minor names, but the latter seldom attain to the permanence and importance of the former. Thus the vast majority of minor names never appear in medieval documents. It is, on the one hand, impossible to obtain a complete set of minor names for any district and, on the other, almost equally impossible to obtain for any particular minor name a continuous series of early forms such as is the basis for the investigation of an ordinary place-name. Furthermore, in many cases minor names are of an impermanent

character (1). Thus, although a few have had a continuous existence for centuries, some modern ones cannot be traced backwards into the Middle Ages, and others once existing have now disappeared. The date when minor names arose constitutes a further difficulty in their historical interpretation: many are modern, many are undoubtedly medieval, and some belong to the Anglo-Saxon period. The lack of documentation, however, which arises from the comparative insignificance of these names, prevents certainty in the majority of cases. When all is said, it is safer to keep the field-name evidence separate from the place-name evidence. That is our justification for maintaining this artificial distinction. It need hardly be added that a field-name form does not lose its historical value because it has no modern equivalent.

In the first place field-name material may be used with advantage to amplify or modify the outline suggested by place-name evidence. Even in an area known to have been intensively settled by the Scandinavians it is interesting to discover if the newcomers arrived in sufficient numbers to influence the minor names of the countryside, the names of the woods and fields, and the general topographical and agricultural terms. Where this has happened we may feel sure that we are dealing with an alien population of mass-migration proportions and not with a few military conquerors who usurped the choicest sites. Thus the place-name evidence is often convincingly confirmed. Material of this kind for several counties has already been published by the English Place-name Society (2). For other areas similar results may be obtained. Leicester, for example, was one of the Five Boroughs and the centre of a Danish Army. The surrounding county contains many Scandinavian field-names of which it is not possible here to give more than a few typical examples (3).

ON *holm*r, 'low-lying meadow', is a very common element: in one parish, Hoby, over half-a-dozen such names are found, including *Le Holm*, *Gamelisholm*, *Scrapholm*, *Riholm* and *Thacholm* (1322, Hastings). It may be noticed that *Gamelisholm* also contains the ON personal name *Gamall*. *Thacholm* also appears in Muston (early 13th cent., Rutland) and in Melton Mowbray (*Thakholm*, 1326, IPM).

ON *vangr*, 'a piece of land, especially in the form of strips in an open field', is found even more frequently: *Hamerwonges*, *Prestwonges*, *Mikelwongs* (1322, Hastings), *Ereverde Wongg* (1330, IPM), *Cuttedewang* (1205, FF), *le Holt wong*, *Gorewong* (1302, IPM), *Brentingwong*, *Croklundeswong* (c. 1250, Ch) etc.

ON *buskr*, 'bush': *Rischebuskes* (1322, Hastings), *Mainbuskes* (c. 1275, Ch).

ON *deill*, 'a share': *Mucakerdayle*, *Wythebuskedayle* (1295, Hastings).

Many other ON words, such as *eng*, 'meadow', *flot* (ME *flat*), 'level ground' and *kiarr*, 'marshy ground', occur but there is no need here to multiply examples indefinitely. It is sufficient to state that field-names in Leicestershire fully bear out the impression of a very thorough Scandinavian settlement.

One cannot safely use these field-names to mark the limits or intensity of the original settlement, for the majority of them probably arose only after the Norman Conquest,

<sup>1</sup> Examples abound in every district. Readers will no doubt know of some recently built by-pass which is locally known as the 'New Road' until after a few years, perhaps, another new road in the neighbourhood usurps this appellation. The original 'new road' is then given another popular name. The same gradual change may often be observed in the local, as distinct from the official, names of farms, etc. The point need not be laboured.

<sup>2</sup> See especially the volumes for Nottinghamshire, Northamptonshire, Yorkshire East Riding and Yorkshire North Riding.

<sup>3</sup> For abbreviations see p. 66.



but their frequency in the 13th and 14th centuries shows how common was the adoption of Scandinavian words into the local English speech. This of course is evidence, if indirect, of the powerful Scandinavian element introduced into the native population during the 9th century settlements.

Cheshire is another area where field-names, still largely unexplored, throw light upon the character of the Scandinavian invasions. It is known that during the early years of the 10th century there occurred a large scale Norse immigration into Wirral (4). How heavy was this influx is illustrated by the field-names which, even in their modern forms, preserve ample proof of the intensity of the Scandinavian settlement. In this area the work of field-name collection is, although in its infancy, already yielding valuable results through the enthusiasm of one or two interested helpers (5). The following data are taken entirely from the Tithe Award schedules to show that even modern field-name forms, all in existence one hundred years ago, may contain evidence of the highest historical value. Names such as *Little Holme*, *Holme Croft* (Oxton), *Lowe Holme*, *Higher Holme*, *Top Holme*, *Five Acre Holme* (Prenton), *Oxholme* (Bidston), *Holme hay* (Moreton) and many others are probably to be traced to ON *holmr*. ON *hiarr*, developing into ME *car*, is commonly used to describe low-lying ground: *Carr*, *Old Carr*, *New Carr*, *Carr hay*, *Carr Meadow*, *Carr Lane* (Saughall Massie), *West Carr*, *Bottom o' th' Carrs* (Moreton), *Carr Field Hey*, *Carrbridge* (Oxton and Landican) are again but a few of the many examples. ME *flat* (ON *flöt*) is equally common, appearing in many parishes as *The Flat*, *Little Flat hey*, *Flatbutts*, *Lower Flat*, *Top Flat* etc. *Sour Flats* (Storeton) may also contain ON *saurr*, 'mud'. *Kirk Hay* (Prenton) preserves either ON *kirkja*, 'church', or the Scandinavianized form of OE *cirice*, 'church': in any case the present form is due to Scandinavian influence. Perhaps the most common word in Wirral field-names is 'rake': the frequency of its appearance is noteworthy, e.g. *Rake Hay* (Bebington, Eastham, Noctorum, Prenton, Storeton, Tranmere, Willaston etc.), *Rake End*, *Rake Shoots*, *Rake Inclosure* etc., *et passim*. Its origin is doubtful but in this area it is highly probable that it should be connected with ON *rak* which here may be taken to mean 'a way or path, generally following some natural depression' (6).

Perhaps more interesting are a group of 'thwaites' found in Bidston: *Tasseys thwaite*, *Meadow thwaite*, *East Salt thwaite*, *Spencers thwaite*, *The Cornhill thwaite*, *Thwaite Lane*, *Marled thwaite*, *Whinny thwaite*, *Wilson's Little thwaite* and *the great thwaite*. This word is ON *þveit*, 'a clearing, meadow etc.', but, like so many of the Scandinavian words mentioned above, it passed into the ordinary dialect speech of the area. Perhaps it should not be used as a Norse test-word, i.e. to prove Norse as distinct from Danish settlement, but it is an undisputed fact that 'thwaites' are most common in regions of Norse settlement—Cumberland, Westmorland, Lancashire and West Yorkshire—and are comparatively rare in Danish districts. This is especially interesting since the Scandinavian colonists in Wirral were mainly Norsemen and not Danes.

A similar piece of evidence comes from the field-names of Arrowe, itself a definite proof of Norse influence since it consists of ON *erg*, 'a shieling, a hill pasture', a word

<sup>4</sup> For a detailed discussion of this question see F. T. Wainwright, 'North-west Mercia, 871-924.' *Trans. Hist. Soc. Lancs. and Ches.*, 1942 (to be published at an early date).

<sup>5</sup> In particular I may mention Mrs Anne Anderson, hon. secretary of the Bromborough Society. Readers interested in Cheshire field-names and/or willing to help in their collection are invited to communicate with Mrs Anderson or with the author.

<sup>6</sup> An examination of one or two surviving examples, e.g. Bromborough Rake, suggests that the term, 'rake', was applied to any sloping path or road. To interpret it as a 'defile' or 'narrow passage' seems altogether too strong—at least for the Cheshire usage.

adopted by the Norsemen from the Irish (*airghe*, Gael. *airidh*). This word may safely be used as a Norse test-word because of its Irish origin. In the fields of Arrowe we find *ON erg* still used a hundred years ago in what appears to be its original sense of 'pasture': *Harrisons Arrowe*, *Whites Arrowe*, *Brown's Arrowe*, *Smiths Arrowe*, *Broad Arrowe*, *Youds and Bennetts Arrowe*, *Whartons Arrowe* etc. A few years ago an amateur collector of field-names included an 'arrowe' name with other names like *Triangle* and *Square Field* under the heading of 'shape'! It is quite clear that these names are derived from *erg*, and, incidentally, they provide a striking proof that the interpretation of Arrowe, the village-name, is correct. Again there is no point in producing further examples; it is sufficient to remark, first, that the field-names of Wirral give ample support to the place-name evidence for a very intensive Scandinavian settlement there and, secondly, that there are a few more hints that the Scandinavians were, in general, Norsemen rather than Danes. Finally it may be added that Scandinavian field-names grow less and less frequent as one advances eastwards from Wirral towards central Cheshire, that is to say as one moves out of the area of Norse settlement.

In searching for Scandinavian influence in field-names, however, one vital fact will be noticed. Even in those areas in which we find the strongest evidence of a Scandinavian settlement we also find that English field-names survive in great abundance. This serves to remind us of the fact, presumably accepted but often overlooked, that there remained a numerically powerful English element even in the most Scandinavian parts of Danelaw. In spite of the wholesale adoption of alien words and personal names, and in spite of certain alien sound-developments, the resulting dialects were quite definitely English.

Field-names not only confirm the intensity of Scandinavian influence in areas where settlement is known to have occurred; they also preserve evidence of a 'Scandinavianized' population in districts where it finds little or no record in the ordinary place-nomenclature. Professor Stenton has pointed out that, although 'there are few traces of alien settlement in the village-names of the country between Kettering and Market Harborough' an inspection of the minor names proves that 'the Danish influence in the country where they are found was strong enough to colour the names ultimately given to its remoter woods and fields' (7). Similar examples may be found elsewhere. Of the counties of Huntingdon, Bedford, Cambridge and Buckingham it has been said that 'it would never have been inferred from the place-names of any of these counties that they had once undergone a Danish settlement sufficient to change the whole customary law which prevailed within them' (8). Yet many of the medieval field-names preserve traces of Scandinavian influence. In the Place-name Society volume for Bedford and Huntingdon (p. 300), nineteen examples are listed of Scandinavian personal names found in minor names. Of Essex it has been said that 'place-names show but slight traces of Scandinavian settlement', but it was added that 'some thirty field-names have been noted that contain Anglo-Scandinavian personal names or significant words such as *bigging*' (9). Thus may field-names help to complete the picture even when the evidence they offer is strictly applicable only to a time some centuries after the Scandinavian invasions.

In this brief account perhaps too much stress has been laid upon the field-name contribution to the history of the Scandinavian settlement. Certainly the value of field-name study does not end there. Materials are available for the examination of dialect

<sup>7</sup> *Trans. Royal Hist. Soc.*, 1942, XXIV, 4-5.

<sup>8</sup> PNS, III, xix.

<sup>9</sup> PNS, XII, xxvii.



development, for the identification of names and sites hitherto regarded as 'lost', for the study of medieval agricultural methods and conditions, for the guidance, sometimes, of archaeologists searching for a lost road or fort, and for considerable additions to local histories.

The identification of the 'lost' Domesday manor of *Edelaue* in Cheshire is a case in point. Many years ago Mr Fergusson Irvine noticed in the Willaston Tithe Award a group of more than thirty fields bearing such names as *Adler*, *Big Adler*, *Parsons Adler* etc. These, he suggested, marked the site and preserved the name of the Domesday *Edelaue* (10). Professor Tait (11) accepted this identification, but sufficient credit is seldom given to Mr Irvine's researches of half a century ago, and, incidentally, to the material which made possible this deduction. One cannot doubt that the Domesday *Edelaue* is to be identified with the well-marked cluster of 'Adler' fields seen on the Willaston Tithe Award map: (12) it is as certain as any such identification can be. It may be remarked that the name survives to-day as Hadlow Road, now further perpetuated as the designation of a small railway station. Of wider interest is Professor Stenton's recent identification of the Anglo-Saxon Chronicle *Fethanleag* with a minor name, *Fethelee*, which appears in Stoke Lyne, northeast Oxfordshire, in a final concord of 1196 (13). Under the annal for 584 the Chronicle records a battle between the Britons and the West Saxons at *Fethanleag*. Following the battle of *Bedcanforda* and the capture of Limbury, Aylesbury, Bensington and Eynsham in 571, and following the battle of Dyrham and the capture of Gloucester, Cirencester and Bath in 577, the battle at *Fethanleag* in 584 clearly marks, as Professor Stenton points out, a further stage in the West-Saxon advance. Thus the location of *Fethanleag*, hitherto unidentified, possesses a historical value difficult to over-estimate. It was made possible by a late 12th century field-name, a woodland-name—*bosco de Fethelee*.

Evidence for the existence of ancient roads is frequently preserved in field-names and place-names. The spade, or at least an inspection of the ground, is always necessary for final confirmation, but indications as to where a search might profitably be made are often found in the local nomenclature of a district. OE *stræt*, a loan-word from Latin (*via strata*), was the term usually used to describe a metalled road. Our ancestors differentiated carefully between metalled roads, 'streets', and other tracks or green roads. The numerous Strattons, Strettons, Stratfords, Stretfords etc., contain this very common place-name element, and it has often been noted that these places usually lie on or near Roman roads. Field-names which contain this element often give valuable aid in ascertaining the exact line of a road which has been lost or obliterated. It should be remembered, however, that all metalled roads were not Roman roads, even in the Middle Ages, and that the word 'street' must sometimes have been used to describe other roads. Even so the occurrence of field-names preserving this element certainly justifies a close investigation of the area in which they are found. Any list of the cases in which place-names and field-names record the existence of an ancient road would have little value for it could not but be incomplete. Reference may be made to the publications of the English Place-name Society. Here the purpose is merely to illustrate this important contribution to history. As early as 1771 Whittaker, the historian of Manchester, noted that the names, 'Street Fold in Moston, Street Bridge in Chatherton and

<sup>10</sup> *Journal Chester Arch. and Hist. Soc.*, 1893, N.S. v, 81.

<sup>11</sup> *The Domesday Survey of Cheshire*. Chetham Society, 1916, LXXV, p. xiii.

<sup>12</sup> In Neston parish church.

<sup>13</sup> *Trans. Royal Hist. Soc.*, 1940, XXII, 19-20.

Street Gate in Ryton', pointed to the existence of a Roman road from Manchester through Moston, Chadderton and Royton to Littleborough. This road does not appear on the O.S. map of Roman Britain, presumably for the very good reason that no traces of it now remain above ground. Since much of this area is 'built up' it is perhaps not surprising that traces of the road have been destroyed, but further east, on the open moorland near Littleborough, it is clearly visible, and from this point its course, north-eastwards to Ilkley, is marked upon the map. Without making a personal examination one may say that the 'street' names quoted above suggest very strongly that the Roman road seen on Blackstone Edge continued through Littleborough, Royton, Chadderton and Moston to approach Manchester by a direct route (14). Further evidence may well be forthcoming when the minor names of this district have been studied minutely.

Another Lancashire example may be quoted (15). It may be regarded as certain that the Roman road which runs from Elslack down the Ribble Valley—marked as far as Ribchester on the Map of Roman Britain—proceeded westwards even beyond Preston, and at least as far as Kirkham. In Dowbridge, within a mile of Kirkham, we find a 13th century reference to a *magna strata*, and further east in Lea we find *Wattelingestrete* (1285) and a field called *vattelingstrete* (c. 1300). Watling Street Road in Fulwood, Preston, no doubt to-day marks the line of this road. Sir Allen Mawer (16) has explained why the names Watling Street, Ermine Street, Fosse Way and Icknield Way were readily adopted by other roads. The above is another example to be added to his list of 'Watling Street' names, and it is also a good example of the historical value of field-name study. A collection of 'street' minor names, which abound in many districts and are usually found in close association with Roman or other ancient roads, would obviously throw new light on road-systems destroyed by the demands of agriculture or by industrial development. The ancient roads of Wirral, for example, are very obscure. Suggestive fragments only have been recorded, but further evidence is preserved in the field-names. The time is not yet ripe for any attempt at co-ordination although many scattered indications have already appeared. In passing it may be remarked that names like *Street Hey*, *Big Street Hay*, *Street Hay Meadow*, *Street hey croft* and *Pavement Field* (in the Willaston Tithe schedule) are certainly sufficient to make desirable a thorough examination of the sites.

References to hill-forts and to other ancient sites are often contained in place-names and field-names. Mr O. G. S. Crawford (17) has shown the potential archaeological interest of such elements as *sead*, *stodfald*, *tunsteall*, *burh* etc. Sir Allen Mawer (18) has pointed out the significance of the elements *tot*, in numerous Toot Hills or 'look-out hills', *tot-ærn*, 'look-out house', and *weard-setl*, 'watch-seat', which appears in Warhill Top, Wassel Grove etc. *Burh* is the ordinary OE word for a prehistoric hill-fort although it is also used to describe any fortified place or even a manor house. It is a common element in field-names as well as in place-names, and often it is the sole

<sup>14</sup> F. T. Wainwright, 'The Anglian Settlement of Lancashire', *Trans. Hist. Soc. Lancs. and Ches.*, 1941, xciii, 15 note 1 and map.

<sup>15</sup> *Ibid.* p. 15, note 3, for details and references.

<sup>16</sup> ANTIQUITY, 1927, 1, 153. In this article, 'Place-names and Archaeology', some interesting examples of the names of roads or tracks are given, e.g. *Ferdway*, 'the fyrd or army road', *Chep-tingwey*, 'market-road', etc.

<sup>17</sup> 'Place-names and Archaeology', *PNS*, I, part I, 143-64.

<sup>18</sup> 'Place-names and Archaeology', ANTIQUITY, 1927, 1, 151-8.



## FIELD-NAMES

remaining record of a once important site. In 1283 (IPM) there occurs *Burg*, 'a member of Groby', Leics., which in all probability refers to an Early Iron Age hill-fort in the neighbourhood (19). *Aldebury* or *Oldebury* is a name which is found frequently; each example deserves careful notice. More definite references to prehistoric forts are the 'earth burhs'. The usual medieval forms for Burrow on the Hill (Leics) are *Burg*, *Burgh* and the like, but there exists a distinct series of forms which contain OE *eorþ*, 'earth':

1251	CUR LEI	<i>Erburg</i>	1285	VAR COLL.	<i>Erthburg</i> (p)
1260	CUR LEI	<i>Hereburg</i>	1326	IPM	<i>Erdburg</i>
1262	GRAV	<i>Erburgh</i>	1343	BM	<i>Erdburghe</i>
1292	IPM	<i>Erburg</i>	1355	MISC.	<i>Herdeburgh</i>

Clearly there must have been an ancient earthwork at Burrow on the Hill (20) for the reference is to the earthen ramparts. Many similar examples are known. In Wiltshire (21) are found OE *eorðbyrig* (BCS, 600), *Orthbury* (13th cent.), *Erthbery* and *Orebiry* (*temp.* Henry III), and in Stepney, Middlesex (22), an *Erthbery* (13th cent.) has been noted.

*Stodfold* occurs occasionally as a minor name and it has been suggested that OE *stodfald*, 'an enclosure for horses', was sometimes applied to Roman forts and to other ancient enclosures such as stone circles (23). It is not necessary to assume with Professor Ekwall (24) that these old sites were actually used as horsefolds, for a similarity in appearance or a popular guess at their origin may well account for the description of earlier sites as *stodfolds*. The same significance may sometimes attach to other *-fold* names, e.g. the common *Pinfold*. At least 'all such names when occurring away from any known earthwork may conceal ancient remains, and should be noted for observation on the ground when an opportunity occurs' (25). *Stodfold* in Husbands Bosworth, Leics. (*temp.* Henry III, Add. Ch. 28639), is an hitherto unnoticed example which may be added to the lists given by Mr O. G. S. Crawford.

Many other elements in place-names and field-names are of interest to the historian and to the archaeologist. For information on the more important of these the reader is referred to the articles of Sir Allen Mawer, Professor F. M. Stenton and Mr O. G. S. Crawford and to the publications of the English Place-name Society.

The study of place-names and field-names often permits the identification of folk-meeting places. The sites which are recorded in place-names are widely known: Thingwall (ON. *þingvöllr*, 'assembly field'), Spetchley (OE *spæc leah*, 'speech leah'), Spellow (OE *spell hlauw*, 'speech hill') and Skirmett (a Scandinavianized OE *scir (ge)mot*, 'shire moot') are only four of the many possible examples. Other folk-meeting places have been recovered by the evidence of field-names but this class of nomenclature is still largely unexplored. It is certain that many more sites lie hidden in the minor names of areas which have not yet enjoyed a thorough examination. A new example has recently come to light. Incidentally, also, it demonstrates the value of a collection of field-names as modern as those found in the Tithe Awards, which are, in any

<sup>19</sup> Perhaps Bury Camp, some two miles away.

<sup>20</sup> Burrough Camp is still an impressive earthwork. It is described in *Victoria County History of Leicestershire*, I, 247.

<sup>21</sup> PNS, XVI, 424.

<sup>22</sup> PNS, XVIII, 196.

<sup>23</sup> O. G. S. Crawford, 'Place-names and Archaeology', op. cit. 150-2.

<sup>24</sup> *Dictionary of English Place-names*, 423.

<sup>25</sup> O. G. S. Crawford, op. cit. 152.

case, useful as bases for the exact location of earlier field-name forms. In the Tithe Award schedule for Brimstage, Cheshire, a number of fields called *Mutler* were noted. There were sixteen in all (nos. 196-208, 213, 214, 214a), and they formed a well-marked cluster on the southern boundary of the parish. At once it seemed probable that we were dealing with an ancient place of assembly, a 'moot hill', OE (ge)mot) hlaw. A search in the Tithe Award schedule of the neighbouring parish, Thornton Hough, provided what was expected, four fields called *Mutlow* (nos. 8, 9, 10, and 11) (26) on the northern boundary and adjacent to the *Mutlers* of Brimstage. The accompanying map, a combination of copies of the two Tithe Award maps, shows clearly the position of the moot-hill, the existence of which has been so long recorded in field-names. It only remained to visit the site (27). The *Mutlers* were located on a gently sloping hill between Brimstage and Thornton Hough. The name 'Motler' is still current among local farmers. The Brimstage *Mutlers* have been bisected diagonally by a new road, Talbot Road, and the Thornton Hough *Mutlows* are now covered by the residence and grounds of Lord Leverhulme. The site has not been examined carefully, but the labour entailed in such an examination might well provide its reward.

It is not possible in this brief article to illustrate more than one or two aspects of the field-name contribution to history. Much of our knowledge of the distribution and organization of Anglo-Saxon heathenism is derived from field-names (28), while at the other end of the chronological scale the modern historian will find many details of interest. The once common practice of marling light soils is evidenced by *Marled Hey* and similar minor names which abound in some districts. The name *lym pittes* or *lymputes* (1325, 1326, IPM) in Thurnby, Leics. is noteworthy. The positions of many vanished mills may be recovered from the very common names, *Mill Field*, *Mill Lane* and the like. Older examples may be of greater historical interest, e.g. *Erlesmulne* in Nailstone, Leics. (early 13th cent., Hastings); *Aldemulne* or *Holdmylne* in Knipton (1252, Misc.) was in 1252 already regarded as old, perhaps by comparison with a more recent mill in the neighbourhood. A collection of such references to mills would provide valuable statistics for the economic historian, especially as many of them are not otherwise recorded. Field-names sometimes preserve evidence of a local dispute over land: OE (ge)fliten, 'disputed', is a common element and is found in such names as *fflitlond* (in Harlestone), *Flytlond* (in Benefield), *Flyttelondes* (in Easton), *Flitehyl* (in Isham), *Flithil* (in Broughton), *Flytehil* (in Irthlingborough), *Flithul* (in Braybrooke) etc. (29). The same element apparently occurs in *Flitteris Park* (in Oakham, Rutland) on the Leicestershire boundary (30). The Norman French word, *calenge*, is also applied to land in dispute, and *Le Chaleng* is not uncommon as a minor name. In Barrow on Soar a wood called *le Chaleng* (1239, CUR LEI) may have received its name from the fact that it was the cause of a lawsuit between the Earl of Winchester and the Earl of Arundel.

For the student of dialect, local words abound in field-names, e.g. *Diglakes* (Brimstage Tithe Award) and *Dig Meat* (Landican Tithe Award) in Cheshire contain the dialect word for 'duck', and there is a mass of material for the study of sound development.

<sup>26</sup> One of the four is *Mutlon* (sic), but this is apparently an error.

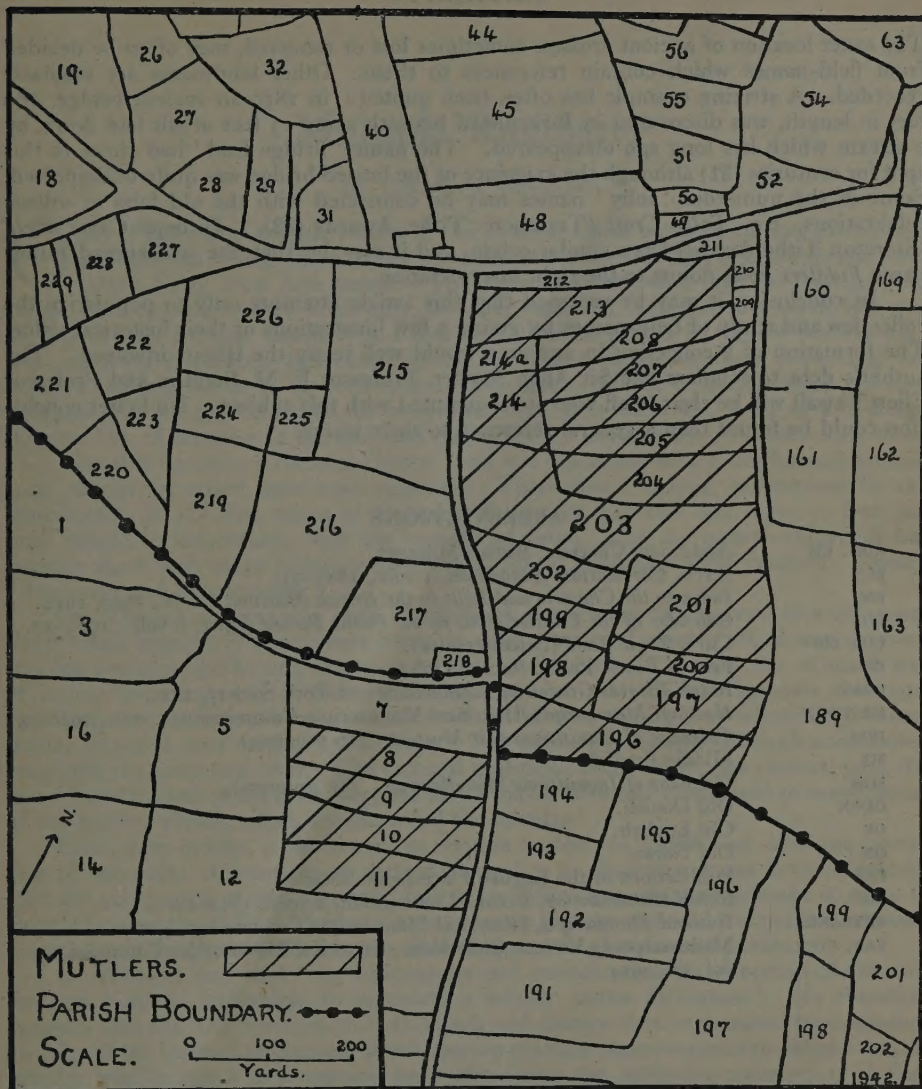
<sup>27</sup> I am greatly indebted to Mrs Anderson who located the *Mutlers* and extracted the modern name, *Motler*, from the local farmers.

<sup>28</sup> cf. F. M. Stenton, 'The Historical Bearing of Place-name Studies: Anglo-Saxon Heathenism'. *Trans. Royal Hist. Soc.*, 1941.

<sup>29</sup> PNS, X, 84-5.

<sup>30</sup> The forms for Flitteris Park are: 1275 RH, *bosū de Fliares*; 1300, etc. IPM, *Fliterys*; 1347 IPM, *Flitefriuh*.





THE 'MUTLER' FIELDS OF BRIMSTAGE AND THORNTON HOUGH  
(From the Tithe Award maps)

## ANTIQUITY

The exact location of ancient crosses, sometimes lost or removed, may often be decided from field-names which contain references to them. Other landmarks are similarly recorded. A striking example has often been quoted: in 1850 an ancient bridge, 100 feet in length, was discovered in Birkenhead beneath some 13 feet of silt laid down by a stream which has long ago disappeared. The name 'Bridge End' had clung to this spot for centuries (31) although the existence of the buried bridge was quite unsuspected. Some of the numerous 'folly' names may be connected with the old fairs or village celebrations, e.g. *Folly Croft* (Tranmere Tithe Award) (32). Perhaps *Caper Field* (Storeton Tithe Award) has a similar origin, and it may be that the widespread minor name *Fiddlers Folly* points to the right interpretation.

In conclusion it may be repeated that this article attempts only to popularize the collection and study of field-names by giving a few illustrations of their historical value. The formation of a collection in any area would well repay the labour involved. The author's debt to scholars like Sir Allen Mawer, Professor F. M. Stenton and Professor Eilert Ekwall will be clear to all who are acquainted with this subject. No better conclusion could be found than a general reference to their works.

### ABBREVIATIONS

ADD. CH.	Additional Charters, British Museum.
BCS	Birch, <i>Cartularium Saxonicum</i> , 3 vols., 1885-93.
BM	<i>Index to the Charters and Rolls in the British Museum</i> , 2 vols., 1900, 1912.
CH	<i>Calendar of the Charter Rolls in the Public Record Office</i> , 6 vols., 1903-27.
CUR LEI	Curia Regis Rolls (Leicestershire).
FF	Feet of Fines, Public Record Office.
GRAV	<i>Rotuli Ricardi Gravesend</i> , Canterbury and York Society, xxxi.
HASTINGS	<i>Hastings Manuscripts</i> , Historical Manuscripts Commission, 3 vols., 1928-34.
IPM	<i>Calendar of Inquisitions post Mortem</i> . (In progress).
ME	Middle English.
MISC	<i>Calendar of Inquisitions Miscellaneous</i> . (In progress).
ODAN	Old Danish.
OE	Old English.
ON	Old Norse.
PNS	Publications of the English Place-name Society.
RH	<i>Rotuli Hundredorum</i> , Record Commission, 2 vols., 1812-18.
RUTLAND	<i>Rutland Manuscripts</i> , Historical Manuscripts Commission, 4 vols.
VAR. COLL.	Manuscripts in Various Collections, Historical Manuscripts Commission, vol. VII, 1914.

<sup>31</sup> cf. *Bridge End* and *Bridge Meadow* in the Birkenhead map of 1823-4.

<sup>32</sup> Other interpretations are possible.



# Multiple Ramparts

by COLIN A. GRESHAM

ONE of the many notable results of the Maiden Castle excavations was the formulation of the 'Sling-stone theory' to account for the building of multiple ramparts round Early Iron Age camps. The excavations revealed that the Iron Age A2 camp, at first on the eastern knoll only, but later extended to include the whole hill, was surrounded by a single rampart, and that this, at some period in the 1st century B.C., was suddenly reinforced and encircled by two outer ramparts, thus producing the massive defence works to be seen today. The nature of the construction of the new defences led the excavators to the conclusion that they were the work of a 'foreign dynasty' (1), of invaders of the fort, and not of the A2 inhabitants. It was found, however, that this 'sudden structural change' was not associated with a similar sudden cultural change, as might have been expected. There was a change, represented by the introduction of the first traces of Iron Age B pottery, but this was slight at first and only gained in importance with the passage of time. The A2 pottery remained but became more and more influenced by the B forms until the latter gradually became 'dominant and characteristic'.

Dr Wheeler explains this state of affairs by postulating 'the arrival of a comparatively small number of newcomers bringing with them new ideas of attack and defence, but not accompanied by any extensive train of craftsmen'. The new idea of attack was of course the sling, and of defence the multiple rampart. These invaders overcame 'the large but unenterprising population of the Wessex downs', on whom they immediately imposed their methods of warfare. The native population, although subjugated, remained the same and retained its cultural traditions except that 'the potters, with the usual imitativeness of the craft, adapted the forms of the metal vessels and to some extent of the pottery vessels which accompanied the invader'.

Such, very briefly, is the theory in relation to Maiden Castle and, with the exception of one point, it seems to be perfectly sound. As, however, it has become evident that the acceptance of this theory in full raises difficulties when an attempt is made to carry it beyond the walls of Maiden Castle, it seems that the one doubtful point mentioned above should be discussed before any final judgment is given on the matter.

Dr Wheeler says that 'the suddenness and instant maturity of certain of the new features make it impossible to postulate a wholly native derivation'. He therefore suggests that the new methods of both attack and *defence* were introduced from abroad. In spite of the internal evidence it is not easy on external considerations to believe that an invader who brings a new weapon must also bring the defensive counterpart to that weapon. In the light of similar occasions when man has devised yet more deadly weapons for the quicker extermination of his kind, it would seem that it is usually the attacked who produces the defensive measures and not the attacker.

In view of this it appears desirable to examine more closely the reasons which led the excavators of Maiden Castle to believe that the builders of the multiple ramparts

---

<sup>1</sup> This and the following quotations, unless otherwise noted, are from Dr Wheeler's paper 'Iron Age Camps in northwestern France and southwestern Britain'. *ANTIQUITY*, XIII, 58-79.

were invaders. The evidence is of course not yet all before us, but we know for certain that a sudden cultural change does not form part of it. Dr Wheeler has emphasized the fact that the new fortifications were associated only with the slight first appearance of B pottery. This is most clearly demonstrated in the quarry which was dug behind the inner rampart to provide material for its reconstruction, for here occupation sites were at once commenced and continued over a long period, so that stratified material was laid down to the depth of some 6 feet. Here we are told, 'the earlier pottery in the quarry was predominantly of Iron Age A2, but from the outset included occasional Iron Age B types, these gradually increased in number . . .' (2). The chief reason then seems to have been the nature of the construction of the new rampart which was built over the A2 rampart. This 'with its elaborate structural walls of chalk and limestone differed as markedly from its predecessor in structural tradition as in size. The use of an exposed revetment of chalk blocks—which rapidly disintegrate on exposure to the weather—behind the crest of the new rampart indicates inexperience of local material on the part of new-comers accustomed to masonry construction. The speedy replacement of the chalk revetment by one of limestone, brought from a distance of upwards of two miles by these determined masons, is obviously and vividly the first-fruit of local experience' (3).

Here Dr Wheeler has a strong argument to account for the facts, but it does not appear to me to be the only possible one. Let it be assumed for a moment, and apart from these chalk blocks I can see nothing against such an assumption, that the builders of the new ramparts were the local inhabitants of the A2 fort. Then it is not beyond the bounds of probability that, if they were constructing new defences under the imminent threat of foreign invasion, they would use local material, quickly obtained, for the work, even if they knew it to have only a temporary value, and later perhaps in a lull go further afield for more durable stone. To this it is objected that the natives of the chalk country had no tradition of stone building on which to work; but it is not wholly necessary to prove a tradition of stone building for the use of chalk blocks as a retaining wall, for they may be a makeshift attempt to use a local material in a new way. The fetching of the limestone from a distance could then be the first-fruit not of 'local experience of masons using a new material' but of first-hand experience of non-masons trying to use such material as was readily obtainable, in imitation of a foreign tradition. The building of the new rampart is associated with the first appearance of Iron Age B pottery and it is possible to interpret the arrival of this pottery as evidence of cultural influence coming from the southwest; this was not necessarily the hostile influence which brought the sling from abroad, but a more local one bringing the knowledge of how stone could be used to revet the inside of ramparts (4).

Since these internal revetments of the ramparts are so important in connexion with the interpretation of the evidence from Maiden Castle, it is necessary to discuss what useful purpose they served. Dr Wheeler explains (5) that the innermost of three chalk block revetments rose to a height of 6 feet or more 'forming an exposed facing on the inner side of the crest'. This was later rebuilt in limestone. At a subsequent renovation

<sup>2</sup> *Antiquaries Journal*, 1936, xvi, 281.

<sup>3</sup> *op. cit.* p. 271.

<sup>4</sup> Excavations in the Cornish cliff-castles may give useful evidence on this point, but attention may be drawn to two promontory forts in South Wales, where internal revetments have been found in connexion with a pure Iron Age B culture. 'Sudbrook', in *Arch. Camb.*, 1939, xciv, 42-79. 'Knave', *op. cit.* 210-19.

<sup>5</sup> *Antiquaries Journal*, 1936, xvi, 273-4.



of the defences, 'when this stone using tradition had apparently been forgotten' the revetment was replaced by a timber palisade again sited on the inner crest. The result that the builders were aiming for was a steep inner face to their rampart, but this, it is pointed out, would neither have hindered the attacker from reaching the top of the rampart from without, nor shielded the defenders standing on top of the rampart. Dr Wheeler assumes therefore that the revetment was not a strategical work at all, but 'can only have been devised to prevent the easy egress of livestock within the camp' (6). Surely these elaborate works, later entailing the transportation of large quantities of stone, cannot be simply dismissed as mere cattle walls, especially in view of the fact that such constructions were not found necessary in the earlier camp? The proper place for the cattle would be in the middle of the camp in pens and not wandering loose amongst the huts and hearths which clustered so thickly in the quarry, the one place in the camp where a little shelter from the weather could be got. This leads to a second suggestion made by Mr A. W. Clapham that the revetments and palisades served 'as an additional wind break on this stormy site' (7). This I believe is approaching the true interpretation which, as I see it, is that the steepening of the inner bank served as a shelter not so much from the wind as from the sling-stones which might come whistling over the top of the rampart. If the new defences had been finished off inside, as in the pre-sling-stone age, with a long tapering slope, anyone standing behind them would have been in danger of a direct hit from a stone just clearing the crest. But the provision of a nearly vertical wall in that area would have given shelter to the defenders and a place in which they could safely wait before going over the top. This, I think, accounts for the fact that there are no revetments in the outer face of the rampart, where they would have offered protection to the invader. In this area the long smooth slopes presented the greatest hindrance with the least cover. It may be urged that the best place for protective walls would be on the crest of the rampart, and certainly when the defences were renovated the timber palisade was placed much higher up, but a study of the tactics of sling warfare may yet reveal reasons for keeping the top of the rampart clear.

If this interpretation of the use of the revetments is correct, they are then to be considered not as an isolated phenomenon, but as part of one great defensive scheme constructed to withstand the new sling-stone menace. Whoever the people that designed and carried out this scheme may have been, they must have been well grounded in the tradition of hill-fort building, but, if the theory stated above be accepted, they need not also have been a race accustomed to the regular use of stone. The chalk block revetment seems to have been only a part, and at first not a very successful part, of a scheme the chief factor of which was the multiplication of ramparts. The conclusion that I should like to draw from this is that the builders of the new defences were a people who had a long acquaintance with the structural problems of building large hill-forts and who, about the time that they were faced with a new danger calling for a greater over-all depth of rampart protection, also came in contact with a protective measure against the same danger which, as they first learnt it, was based on a stonework construction.

This conclusion appears to fit the cultural side of the story closely enough to be acceptable without having to postulate any lost evidence, such as metal vessels which have entirely decayed, or conquerors who could impose vast defensive systems, but not the simple potters' wheel. The pottery sequence at Maiden Castle shows that the A2 people remained there after the building of the new defences with at first only slight

<sup>6</sup> *Antiquaries Journal*, 1936, XVI, 274, footnote.

<sup>7</sup> *op. cit.*

additions to their culture and they, if anyone, had a splendid tradition of hill-fort building of single rampart type stretching back over at least two centuries. A people who had built the first Maiden Castle and then extended it to include the whole hill would surely have had the technical skill and imaginative conception needed to carry out the great rebuilding. As has already been suggested the Iron Age B pottery with its southwestern origins may reflect the influences which led to attempted stone construction. In this connexion it is significant that at a later renewal of the defences the stonework was replaced by a timber palisade. Dr Wheeler interprets this by suggesting that his invaders had forgotten their stone using tradition, but it is possible to see in it the abandonment of new, only partly understood, materials which had not proved satisfactory for another more familiar form of protection, perhaps based on the practical experience of a sling-stone siege.

It is not the purpose of this paper to put forward any far-reaching or revolutionary theory in regard to the complicated developments in southwest Britain during the 1st century B.C., but merely to suggest very tentatively that certain of the facts brought to light in the Maiden Castle excavations may possibly bear other interpretations than those hitherto based on them. It would indeed be rash to speculate any further before it is seen whether these new suggestions will stand the force of expert criticism based on the detailed knowledge of further facts. However in conclusion it may be said that the weight of evidence, external to that of Maiden Castle, appears to be in favour of a more westerly area for the inception and development of multiple ramparts, which later spread northeast through Somerset to the Severn Valley and beyond (8). Dorset may only have received a backwash of this movement and it may in fact be no coincidence in my theory that Iron Age B influence reached Maiden Castle just at the time that the danger of foreign attack became acute. That same menace which led to the rebuilding of the ramparts may also have been the force which caused the B culture to move rapidly north and east. Some such explanation as this may also answer the objection of Mr B. H. St. J. O'Neil (9) to the date 56 B.C. for the rebuilding of Maiden Castle. This date he points out does not leave sufficient time for the 'Hill-fort B culture' to move northwards and pass through the various periods noted at Ffridd Faldwyn Camp near Montgomery. If, as is here suggested, Maiden Castle were not in the main line of development, the date can still hold good, for there is room to the west in both space and time for the development of multiple ramparts.

---

<sup>8</sup> Christopher Hawkes, 'Hill-forts', *ANTIQUITY*, 1931, v, 77.

<sup>9</sup> *Arch. Camb.*, 1942, xcvi, 16-17.



# The Crane Dance in East and West

by EDWARD A. ARMSTRONG

IN a well-known passage Plutarch (quoting Dikaiarchos) tells us that when Theseus landed in Delos with Ariadne, daughter of Minos, on his return from Crete, he and his young companions danced a dance which was called 'The Crane'. This dance, as I hope to show, was an integral part of a cultural pattern of sacrificial and funerary ritual which was widespread throughout Europe and Asia and extended in modified form at least as far as Malekula in the New Hebrides. Unquestionably it was associated with the Labyrinth. Its winding figures represented the hero's wanderings in the Labyrinth on Crete.

Many students have been enticed into endeavouring to discover the mysteries of the Labyrinth and there has been a good deal of speculation as to its ritual significance. It appears in an incipient form on Egyptian seals and the crenellated wall in these representations reappears on Elamite seals. Animals are shown within the walled enclosure, and on the palette of Narmer, a bull, and what is presumably a human sacrificial victim, also appear. Mrs C. N. Deedes has marshalled evidence which shows that the evolution of the complex Labyrinth can be traced from these simpler beginnings to the layout of the Egyptian pyramids with their associated temples (1). The classical writers refer to the temple of Amenemhet III beside his pyramid in the Fayum as the Labyrinth and Pliny claims that Daedalus used this temple as his pattern.

But Mrs Deedes believes that pyramid and temple should be considered as one complex building and points out that the inner chamber of the pyramid of Amenemhet III is concealed by labyrinthine passages. These, she maintains, were designed, not to deceive and frustrate robbers but for ritual purposes. Chinese 'spirit walls' as Mr W. F. J. Knight has pointed out, have an analogous function. They are placed immediately inside the entrance to towns, temples, tombs and houses to thwart marauding spirits which are believed to travel only in a direct line. There is reason to believe that the ritual enacted in connexion with these passages was a representation of the Osiris myth. This myth, in its turn, appears to be a narrative of that ceremonial which was the basis of ancient ritual patterns and may be traced in the myth and ritual of so many peoples the world over—the sacrifice of the king-god in order to ensure the fertility of the land, the fecundity of plants and animals, and the vitality and well-being of the people.

It is often difficult and sometimes impossible to distinguish funerary from fertility elements in ancient ceremonial, as rites connected with the survival of death or the reincarnation of the king (or sacrificial victim) are bound up with ideas of the maintenance or acquisition of vital power. Indeed, our terms such as fertility, vitality, power and the like are more exact than the conceptions such as *mana* used by primitive folk. 'Blessing', conceived with a much more materialistic connotation than it has amongst us describes the aim of these ancient life and power bestowing ceremonies. By imitative magic life-giving powers were conjured up to combat the forces of death, whether represented in the demise of an individual or the decay of life in the fall of the year. Thus in the *Satapatha Brahmana* (2) the sacrificer says: 'This death is the same as the year'.

<sup>1</sup> C. N. Deedes in *The Labyrinth*. Ed. S. H. Hooke, 1935, pp. 3-42.

<sup>2</sup> A. M. Hocart, *The Labyrinth*, p. 270.

So also the Eleusinian mysteries were rites of personal initiation as well as fertility ceremonial. It was natural that rites representative of the sun's energy and the vitality of living creatures should be celebrated on the occasions of sacrifice and death; thus dancing is not unusual at funerals for ancient death ceremonies were re-birth rather than mourning rites. Mourning dances similar to those depicted on paintings of the 18th-20th Dynasties are still performed in Egypt. There is thus no reason to be surprised that dances were performed in connexion with the Labyrinth and its apparently grisly practices. To this day on Malekula maze dances play an important part in the life of the people and the Labyrinth's function is to thwart the uninitiated and ensure that only those who, through performance of the requisite ritual have the necessary knowledge, may find access to the land of the dead; and so recently as 1866 a writer in *Notes and Queries* refers to himself and other villagers playing May-eve games in Alkborough maze 'under an indefinite persuasion of something unseen and unknown co-operating with them'.

The association of the bull—a sun-symbol *par excellence*—with the Labyrinth from very early times should be considered in relationship to the possibility that the Labyrinth is an elaboration of the swastika, one of the earliest and most widespread symbols of the sun. Professor A. B. Cook has argued convincingly that the Cretan labyrinth dance was performed on an arena of swastika pattern. There is, therefore, every indication that the Labyrinth ritual was designed to enlist the fertility and death-defying powers, especially those connected with the sun, on man's behalf.

As for the Minotaur himself he was almost certainly a personage who danced and performed ritual garbed in the hide and horns of a bull. Since Talos, the fierce guardian of Crete, who clutched people in his bronze arms and leaped into the fire, was described as a man in one account and as a bull in another, he may have been the Minotaur. Professor Cook's suggestion that the Minotaur was a Knossian prince masquerading as a bull has to be considered in the light of recent discovery that Melanesian beings corresponding to the Minotaur are not king-gods, but hostile monsters on the wait to devour men's ghosts. I hope it will not be thought fantastic to suggest, in view of the evidence surveyed in the latter part of this essay, that the Minotaur may have had a Chinese counterpart. There are two personages in Chinese mythology who are spoken of and depicted as men with a bull's head, Shên-nung the Divine Harvestman who introduced agriculture and Ch'e-you the inventor of arms and the Master of War. According to Granet he is the forge deified (3). Huang-ti is said to have fought with both of them just as Yü the legendary first king of China vanquished beings who were related to bulls. Some of the details of the dance of Ch'e-you are on record. It is known that the dancers faced each other by twos and threes and sparred with the bullock's horns which they wore on their heads.

The Labyrinth ritual, including the dances, was fertility ritual. It was probably also mortuary ceremonial. Of the many types of magical fertility dances which were (and are) enacted by primitive people we may detect three governing motives which determined the nature of the figures and are particularly relevant to the present discussion. There were ring dances—a very ancient pattern—intended to imitate the movement of the sun and to reinforce (or perhaps in some instances to placate) its power; energetic frolics such as the dances of the Sali in which the participants by jumping or kicking as high as possible sought to stimulate the vegetation and induce vitality and growth in Nature or to vivify the living or the dead; and measures imitating the movements of animals believed to have some power as weather-makers or to be in some way *en rapport*

<sup>3</sup> M. Granet, *Chinese Civilization*, 194 ff.



## THE CRANE DANCE IN EAST AND WEST

with the life-giving powers. The Tarahumare Indians imitate the antics of the deer and the turkey. Carl Lumholtz writes :

For the strange behaviour of many animals in the early spring the Tarahumares can find no other explanation than that these creatures, too, are interested in rain. And as the gods grant the prayers of the deer expressed in its antics and dances, and of the turkey in its curious playing, by sending the rain, they easily infer that to please the gods they, too, must dance as the deer and play as the turkey. (4)

The fact that the Crane dance was a circling evolution suggests that it was a sun dance but its name indicates that probably it had some similarity to the movements of the bird. It is well known to ornithologists, and even to those who have casually visited the Zoo in spring, that cranes perform sprightly and sometimes ludicrous steps when they are displaying ; it is not so well known that there is sometimes a considerable measure of organization and corporate activity when certain species dance. One brief extract from a description of the performance of whooping cranes will make this clear. In her novel *The Yearling*, Margaret Rawlings writes :

The cranes were dancing a cotillion as surely as it was danced at Volusia. Two stood apart, erect and white, making a strange music that was part cry and part singing. The rhythm was irregular, like the dance. The other birds were in a circle. In the heart of the circle, several moved counter-clockwise. . . . The outer circle shuffled around and around. The group in the centre attained a slow frenzy (5).

Although this is a description of the dance of an American species there is no doubt that the European crane is a very considerable performer and in earlier times when it was commoner, and possibly tamer, men would see and wonder at antics of this kind. They would naturally assume that such circling dances, resembling some of their own ring dances, were solar magic. Birds were considered to be wiser than men ; indeed as Aristophanes tells us in *The Birds* at one time they were gods. Possibly the participants in the early crane dance were dressed in crane skins like the Ostiaks in their elaborate crane performance (6). Certainly the practice of dressing up in bird skins was very ancient for figures attired in this way are frequently represented on early Akkadian seals.

There is a further reason why the crane should be associated with solar magic and fertility ritual. References in Homer and Jeremiah show that from the earliest times their wedge-shaped migrating flocks attracted attention. In his counsel to the farmer Hesiod says :

Take heed what time thou hearest the voice of the crane  
Who, year by year, from out the clouds on high  
Clangs shrilly, for her voice bringeth the sign  
For ploughing. . . .

To primitive men the cranes were something more than symbolic of the returning spring ; they brought it. They were the representatives or surrogates of the resurgent sun-god, bringing warmth and fertility in their train. Arriving from the heavens in powerful flight with the red badge emblematic of the sun, lightning, life, blood and virility conspicuous on their heads they constituted a symbol, indeed, an embodiment

<sup>4</sup> *Unknown Mexico*, 1903, I, 330.

<sup>5</sup> This performance may be somewhat imaginatively described but some bird dances are comparatively complex. Cf. E. A. Armstrong, *Bird Display* (Cambridge, 1942).

<sup>6</sup> P. S. Pallas, *Reise durch verschiedene Provinzen des Russischen Reichs* (1778), III.

of exuberant life. Their airy battalions, shuttling back and forth with the changing seasons, were in occult alliance with mighty powers. So, too, as I hope to show elsewhere, were the flocks of geese and swans. These and other magical birds not only portended the weather but controlled it. This is abundantly true of that great ancient rain-maker, the woodpecker; it is also true of the pheasant in China. Thus all the indications are that the crane dance was weather- and fertility-magic and was connected with the Labyrinth, the metropolis of such magic-making, for this very reason.

It was not only at Delos, Troy, and presumably in Crete that there were crane dances. There were dances of white and of black cranes in China. Furthermore they were performed in connexion with a labyrinth and constituted elements in a ritual pattern which it is not difficult to show conformed to the design characteristic of the Ancient East and the Aegean.

Ho-lu, king of Wu, offered his daughter a fish of which he had already eaten a portion, thereby apparently breaking a food-communion taboo. She committed suicide and in order to propitiate her spirit and avert evil consequences the king sacrificed dancers by burying them alive. He constructed a magnificent tomb for her remains, furnished with objects of great value. An underground passage, the prerogative of royalty, led to the sepulchral chamber. The dance of the white cranes was enacted in the market-place of the capital. The crowd was ordered to go and see the spectacle and to follow the cortège. It was so arranged that boys and girls entered the subterranean passage with the crane dancers. Then a machine, such as that used at the burial of Ch'in Shih Huang, was set in motion to close the passage and bury the dancers and their followers alive (7).

It is apparent that the ceremonial resembled that at the funeral of Ch'in Shih Huang. What happened on that occasion?

The mighty warrior was laid to rest under a gigantic tumulus—the largest artificial hill in the world, I suppose, 500 feet high and about two miles in circuit—in a labyrinth of underground passages, beside what is described as a 'sea' of quicksilver, intended to preserve the body for all time against decay, with many hundreds of women and slaves of his court and a fabulous mass of treasure. And when the work was done lest after ages should ever know the clue to that labyrinth, all the workmen, ten thousand in number who had been employed in its construction, were driven into the underground passages and the openings closed for ever. That tumulus is there to this day, a few miles from Hsian in Shensi, for whoever shall care to explore its recesses, and whilst its construction is, as I said, the supreme and crowning instance on record of this sort of barbarity, it is only the greatest example among many. . . . All through Chinese history we may find traces of the archaic barbarism of which this funeral was the crowning masterpiece (8).

I have chosen this description, based on the *Historical Record* of Ssu-ma Ch'ien, chapter vi, because the writer, without any intention to emphasize similarities between this ritual and that of the Fertile Crescent and Aegean has stressed the word 'labyrinth'. The resemblance between the funerals of these Chinese kings and the kings and queens of Ur is evident. It is possible that when the tumulus is opened it will be found that the passages were designed on lines which resemble the passages in the pyramid of Amenemhet III. In the *Book of Odes* there is a ballad lamenting the fate of three statesmen who

<sup>7</sup> Wu yüeh ch'un ch'iu, chap. 4; Mu t'ien tzu chuan, chap. 4. J. J. M. de Groot, *The Religious System of China*, I, 726. Cf. the legend of the Pied Piper of Hamlyn.

<sup>8</sup> W. J. Clennell, *The Historical Development of Religion in China*, 1917, pp. 31 f.



went down into the burial pit of Duke Hu in like manner as Queen Shub-ad's attendants followed her to the grave.

There seems to have been considerable similarity in detail between the Delian and the Chinese crane dances. Lucian makes it clear that the performance was by a number of dancers together and therefore different from the solo dancing of his own day. On the *krater* of Klitias and Ergotimos, Attic work of 600-500 B.C., Theseus is shown leading the dance which is performed by seven youths and seven maidens (9). Two groups of eight dancers executed the Chinese dance. One account says that it was performed on horseback. An Etruscan *oinochôe* from Tragliatella shows seven dancing warriors and two horsemen in front of, and probably issuing from, a labyrinth. Both riders carry shields on which a bird is depicted (10).

The connexion of crane dances with weather magic as well as with sacrificial rites is sufficiently clear, for we are told that black cranes and headless demons holding lances danced during a drought (11).

Granet has shown that the crane dances were in connexion with thunder, rain, fertility and reincarnation—the same complex of ideas as we have seen to be associated with crane dances elsewhere (12). To this day cranes figure in Chinese funeral ceremonies. A picture of one is placed on top of the catafalque when coffins are carried to the grave, and the wheeled sedan chair in the funeral cortège containing an image of the departed is sometimes drawn by a paper crane (13). In South China I have seen a white paper model of a crane suspended from the window of a house in which someone had died. The crane is a very common symbol of long life in Chinese art and is frequently depicted with the pine tree, which has the same significance. The Queen Mother of the West, who seems to be a derivative of the old Mother Goddess type, is shown riding on, or accompanied by, a crane, or alternatively a 'phoenix'—a mythical bird which is undoubtedly a syncretistic fertility symbol. The Patriarch of Taoism is represented wearing a red robe embroidered with white cranes and riding a one-horned monster, not unlike a water-buffalo.

It is not within my present scope to trace the further ramifications of crane symbolism except to point out that what appear to be three cranes are depicted on the solar disk in Japan, and to draw attention to the Celtic crane-bull-willow complex commented upon by Professor Cook. The sacrifice of cranes at the horse sacrifice in Vedic times shows that in India the bird was connected, as in Europe and China, with solar and fertility rites (14).

The evidence which has been brought forward proves that the cultural complex in which crane dances were an element spread over a very wide area in Europe and Asia and that there was substantial identity between the mythology and ritual connected with the crane in China, the Fertile Crescent and the Aegean. I have shown in other papers

<sup>9</sup> Furtwangler-Reichold, *Griechische Vasenmalerei*, I, 60 f, pl. 13.

<sup>10</sup> G. Q. Giglioli, *Studi Etruschi*, 1939, III, III-59, pl. XXVI.

<sup>11</sup> *Huai-nan tzu*, chap. 6.

<sup>12</sup> M. Granet, *Danses et Légendes de la Chine Ancienne*, I, 221 f.; 314, n. 1; II, 504. Unfortunately very little is known of the Dance of the Banded Rail in the New Hebrides. It is, however, the principal dance during mortuary rites. Where a bird-belief passes beyond the range of the bird it is commonly transferred to another species, but the information available is too scanty to warrant the conclusion that the rail is the Malekulan surrogate of the crane.

<sup>13</sup> J. Doolittle, *Social Life of the Chinese*, 1866, I, 193; Doré and Kennelly, *Researches into Chinese Superstitions*, v, 706; N. B. Dennys, *The Folk-lore of China*, p. 25.

<sup>14</sup> Macdonell and Keith, *Vedic Index*, II, 61.

## ANTIQUITY

that the plough ritual of China was similar to that in the West (15) and that the symbolism of the herb mugwort was the same in Europe, China, and to some extent at least, in Mexico. All this is part of a much larger body of evidence which I have accumulated establishing beyond question the migration of a complex of myth and ritual, religion and custom, from the Fertile Crescent over a great area of the world's surface.

### BIBLIOGRAPHY

- CRANE DANCE. Plutarch, *Theseus*, XXI; Julius Pollux, IV, 101; Lucian, *De salt*, 34, 49; Callimachus, *Hymn* IV (to Delos) 307-15; Marius Victorinus, *Ars Grammatica*, I, XVI, 12-14; S. Gregory of Nazianzen, *Oration Theologica*, II, 25; Tryphiodorus, *Taking of Ilios*, 352-57; Hesychius, *Lexicon*, s.v. γέρανος and s.v. γερανούκκος. (I am indebted to Mr R. W. Cruttwell for some of these references).
- MIGRATION OF CRANES. II, III, 3; Hesiod, *Op.* 776; Aristophanes, *Aves*, 711; Jer, VIII, 7.
- THE LABYRINTH (modern authorities). A. B. Cook, *Zeus* (1914-40), I, 472 ff.; III, II, 1086-7; Sir A. J. Evans, *The Palace of Minos at Knossos*, III; Sir J. Frazer, *G.B.*, III-IV, 70-8; S. H. Hooke, ed. *The Labyrinth*; W. F. Jackson Knight, *Cumaeae Gates*; J. Layard, 'Maze dances and the ritual of the labyrinth in Malekula'. *Folklore*, XLVII, 123-70 and *Stone Men of Malekula, Vao*; W. H. Matthews, *Mazes and Labyrinths*; W. O. E. Oesterley, *The Sacred Dance*.
- DANCES OF BIRDS AND DANCES IMITATIVE OF BIRDS. E. A. Armstrong, *Bird Display*; W. D. Hambly, *Tribal Dancing and Social Development*; C. Sachs, *World History of the Dance*, tr. B. Schönberg.
- CHINESE LABYRINTHINE BURIAL. E. Chavannes, *Les Mémoires Historiques de Se-ma Ts'ien*, II.

---

<sup>15</sup> *Folklore* (in the press): The Ritual of the Plough.



# The Origins of the Alphabet

by DAVID DIRINGER

THE alphabet is the most highly developed, the most convenient and adaptable form of writing.(1) It consists of a number of purely conventional signs or symbols called letters, each one denoting a single sound, at least theoretically. Alphabetic writing is now universally adopted by civilized peoples; its use is acquired in childhood with ease—it is obviously much easier to learn 22, 24 or 26 signs than, for example, 45,000, or 10,000, or even 3000 Chinese symbols. The alphabet may also be passed from one language to another without great difficulty. It is no longer an exclusive prerogative of the priestly or other privileged classes, as ancient ideographic scripts were. Education has become largely a matter of reading and writing and is possible for all.

It is this simplicity and adaptability which has secured the triumph of alphabetic writing over other systems; it is generally accepted by scholars that practically all past and present alphabets have a common origin.

No other system of writing has so long a history. Nevertheless, the story of the alphabet from the end of the second millennium B.C. is not hard to trace. It is its pre- and proto-history that is still wrapped in obscurity. The principal problem, still unsolved, is that of its origin. Since classic times this problem has been a matter of serious study. The Greeks and Romans entertained five conflicting opinions regarding the people who may have invented the alphabet—the Phoenician, the Egyptian, the Assyrian, the Cretan, and the Hebrew. In modern times various theories, not very different from those of ancient days, have been current as to its origin. Each country situated in, or more or less near to, the east Mediterranean, has been seriously regarded as a claimant.

The earliest modern view, already held by earlier scholars, was that of Lenormant, published by De Rougé in 1874, that Egypt was the starting place of the alphabet. The Egyptian theory has been subdivided into three theories—the hieroglyphic (Cham-pollion, Lenormant, Halévy); the hieratic (Luzzatto, De Rougé, Taylor, Kyle); the demotic (Bauer). It is generally believed that the Egyptian script once contained a kind of alphabet of 24 letters, but this was not alphabetic writing. Since the earliest times Egyptian writing, in addition to the signs for three consonants, also used signs for bi-consonantal and uni-consonantal words or parts of words. Later the uni-consonantal signs were used very seldom, at any rate much less, and never without ideographic symbols. Furthermore, in a true alphabet each sign generally denotes one sound only, and each sound is represented by a single, constant symbol, while in the Egyptian scripts there existed different signs for the same sound; thus, the same sound could be written in many ways. On the other hand, the Egyptian symbols are so numerous, 604 without the ligatures and numbers, that accidental resemblances are to be expected. Apart from many other considerations, I am unable to believe that if the alphabet originated in Egypt, the Egyptians would have continued to use their old and extremely complicated writing. Furthermore, when they found it necessary, centuries after the introduction of the alphabet, to simplify the hieroglyphic and hieratic scripts, they did not use the

---

<sup>1</sup> S. H. Hooke, 'The Early History of Writing', *ANTIQUITY*, XI, 261-77.

Egypt	Crete	N-S.Alph.	Modern Hebrew	Egypt	Crete	N-S.Alph.

1.

Key	Linear	Sinaitic	Phœnic.	Key	Linear	Sinaitic	Phœnic.

2.

FIG. 1. (1) THE CRETAN THEORY REGARDING THE ORIGIN OF THE ALPHABET;  
(2) THE LINEAR SIGNS THEORY



## THE ORIGINS OF THE ALPHABET

alphabet, but created the demotic script. Recently at any rate a few scholars (Ronzevalle, Dunand, and others), have held the opinion that the alphabet is directly derived from Egyptian writing.

The attempts made to show that the cuneiform scripts (Delitzsch), the Sumerian (Hommel, 1904, or Waddell, 1927), or the Babylonian (Peters, Hommel, Ball, Peiser, Lidzbarski), or the Assyrian (Deecke), or the syllabary of Cyprus (Praetorius, Koenig), or the Hittite hieroglyphics (Sayce), are the true parents of the alphabet, may be regarded as even less successful. The pan-Germans (Wartenberg, Wilke, Wilser, von Lichtenberg), and especially the Nazis (Schuchhardt, Günther), are sure, naturally, that the inventors of the alphabet belonged to the pure Aryan, nordic race.

Sir Arthur Evans, followed by other scholars, developed the theory that the alphabet was taken from Crete to Palestine by the Philistines, and from them borrowed by the Phoenicians. This is obviously impossible; the Philistines conquered the coast of

Cyprian syllabary		N.-Sem. alphabet		Cyprian syllab.		N.-Sem. alphab.	
signs	value	signs	value	signs	value	signs	value
Δ Δ	ya	◁ ◁ ◁	d	𐀀 𐀁	se	𐤁 𐤂 𐤃 𐤄	m
⋈ ⋈	ke	⋈ ⋈ ⋈	h	⋈ ⋈ ⋈	pa	⋈ ⋈ ⋈	s
⊥	fe-ve	⊥ ⊥ ⊥	z	+ +	lo	+ x +	t

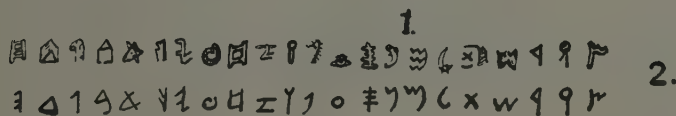


FIG. 2. (1) EXTERNAL LIKENESS OF THE SIGNS DOES NOT INDICATE THEIR DIRECT DEPENDENCE;  
(2) THE PICTOGRAPHIC THEORY REGARDING THE ORIGIN OF THE ALPHABET

Palestine about 1220 B.C. when the alphabet was already some centuries old. The Cretan theory has had recently other adherents (Dayet, Sundwall, Chapouthier) and lastly Grumach (FIG. 1, 1). It is certainly true that some alphabetic signs have a resemblance to Cretan linear signs, but the similarity is only external and not internal, since the Cretan script is as yet undeciphered. Thus the resemblances may be accidental, especially as they concern mainly pure geometric signs which may easily be found in any primitive script. FIG. 2, 1, shows that external similarity does not indicate their direct dependence. However, it is quite possible that the inventor of the alphabet knew something about the Cretan signs, and used some of them quite independently of their meaning.

































A different view is offered by Sir W. F. Flinders Petrie, who argued that both the Phoenician and Greek alphabets, together with those of Asia Minor and the South-Semites, as well as the Cyprian syllabary and some Egyptian scripts, developed from the geometric prehistoric marks employed throughout the Mediterranean area from the earliest times. But Petrie is practically alone in supposing that these marks had any significance, and his theory of the development of various local alphabets from such marks has not found general acceptance. His theory has been recently transformed by

Egyptian hierogl.	Sinai	South-Semitic	North-Semitic & Heb.	Mean.
				ox א
				house ב
				hook, nail י
				hand, bent, hand כ
				water מ
				A fish נ
				snake ב
				eye ע
				mouth פ
				head, tooth ש
				mark ת

	אלה א
	בית ב
	nose-ring ג
	ד (Cowley)
	ה (Sayce)
	ו (Gardiner)
	ז (Cowley)
	ח (Sayce)
	גoad ל
	מים מ
	נחש נ
	עין ע
	קשת ק
	ראש ר
	שן ש
	תה ת
	determinative of goddess

1.

2.

1 	2 	3 	4 	5 	6 	7 	8 	9 	10 	11 	12 	13 	14 	15 	16 	17 	18 	19 	20 	21 	22 	23 	24 	25 	26 	27 	28 	29 	30 	31 	32 				
a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	s'	s <sup>2</sup>	s <sup>3</sup>	t	u	v	w	x	y	z	aa	ab	ac	ad	ae	af	ag

3.

FIG. 3. (1) DR GARDINER'S SINAITIC THEORY; (2) DR COWLEY'S DECIPHERMENT OF THE SINAITIC WRITING; (3) THE CUNEIFORM ALPHABET (XV-XIV CENT. B.C.)

## THE ORIGINS OF THE ALPHABET

T. H. Gaster (FIG. 1, 2). At any rate, it might have been that the great inventor used some of those signs, with which he was perfectly familiar, in the same way as he might have used the above-mentioned Cretan signs.

It had also been argued, by Sir John Evans, from the possible resemblance of a few early alphabetic letters to the objects denoted by their names, that the letters were once pictures used as ideograms. The intrinsic probability of some Egyptian or Babylonian influence forbids the postulate of a totally unknown ideographic system, of which, besides, no trace has come to us. But it is interesting to note that this theory was suggested seventy years ago, when knowledge of oriental epigraphy was extremely slight. FIG. 2, 2, shows Sir John Evans' theory: first line, the 'original ideograms'; second line, the evolved North-Semitic letters.

The Egyptian view was revived in 1916 in papers published by Dr A. H. Gardiner and, independently, by Prof. K. Sethe. Dealing with the early Sinaitic inscriptions discovered in 1904-5 by Sir Flinders Petrie, Dr Gardiner and also Prof. Sethe, came to the conclusion (FIG. 3, 1 and 2) that we have to do with a stage of writing intermediate between Egyptian hieroglyphics and the Semitic alphabet. But Gardiner's classical identification

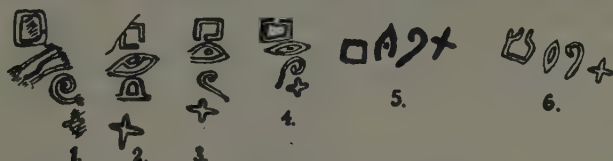


FIG. 4. GARDINER'S CLASSICAL IDENTIFICATION OF THE NAME OF THE GODDESS BA'ALAT IN THE SINAITIC INSCRIPTIONS no. 343 (1), 352 (2), 353 (3), 354 (4), 345 (5), 346 (6)

N.B.—The second sign (downwards) in 1, and the third in 2 are irregular

(FIG. 4) of the name of the goddess Ba'alat, is the only probable one among all the tentative decipherments of the Sinaitic inscriptions, although a most extensive literature of interpretations and comment has been published. Since no categorical conclusions are justified, it cannot be said to have been proved that the proto-Sinaitic writing was the great mother-alphabet. The only reasonable conclusion is that we have in the palaeo-Sinaitic inscriptions one of the earliest known attempts at alphabetic writing.

An epoch-making discovery was made by C. F. A. Schaeffer, G. Chenet and Ch. Virolleaud in 1929 and the following years, at the ancient Ugarit, on the Syrian coast opposite the most easterly cape of Cyprus. At that site there were found clay-tablets, documents of inestimable value in many fields of research, written in a hitherto unknown cuneiform-alphabet of 32 letters (FIG. 3, 3), and deciphered by H. Bauer, E. Dhorme and Ch. Virolleaud. A few texts may be dated as 15th century B.C., but it is difficult to fix the date of origin of the script—possibly in the 16th century. The use of this writing seems to have ceased in the 13th century B.C. although we have evidence only for the 14th. Among the theories on the problem of the origin of this alphabet the most natural one is that it was invented by a native who knew the North-Semitic alphabet, but was accustomed to the use of clay and stylus which were not adaptable to the writing of linear letters. From the former he borrowed the idea of an alphabetic, consonantal writing; from the latter he imitated the wedge-shaped elements, which he arranged in various simple combinations. At any rate, it is probable that the cuneiform alphabet



is artificial and is not an adaptation of other systems, as suggested by some scholars, although direct evidence is wanting.

Other most important documents, discovered by M. Dunand in 1929 and 1933 at Byblos, Syria, present a hitherto unknown type of writing, recognized as syllabic because of the eighty or more characters employed, and presumably bearing some resemblances to Egyptian hieroglyphs. Dunand and Albright date these texts near the end of the third millennium B.C., but it seems that we cannot go beyond the second millennium. Dunand, basing his view on these inscriptions, claims an early Byblian hieroglyphic script as the possible progenitor of the alphabet. Another enigmatic, probably alphabetic, inscription from Byblos of about 1400 B.C., forms, according to Dunand's opinion, a suitable link. But this theory has not found adherents, not having proved entirely satisfactory. One thing is certain, that in Byblos another attempt was made in the first half of the second millennium B.C. to introduce an alphabetic writing.

It is also possible that yet another attempt at alphabetic writing is shown by some inscriptions of the second millennium B.C., in an unknown writing, found in Kahûn and other places in northern Egypt. More important is the enigmatic inscription discovered in 1931 in Balu'a (Moab, Transjordan), perhaps of the early 12th century B.C. This inscription remains undeciphered, and its epigraphic relationship is extremely obscure. Professor Albright's hypothesis of separating the text from the relief, and considering the writing as a variant of the above Byblian script belonging according to him to the third millennium B.C., is ingenious but not very probable, while Dr Gaster's comparison with other Semitic scripts is not convincing.

All these, mainly Semitic, and perhaps some other attempts still less known (some graffiti discovered at Ur, a seal inscription possibly of Asia Minor, etc.), were partly independent, but practically they had the same aim—to create a simpler means of communication than those which were already in use. This existence of many different attempts with the same purpose is one of the reasons why it is so difficult to determine the origins of alphabetic writing.

The solution of the enigma may come from Palestine, where several middle and late Bronze-age inscriptions have been discovered recently. Their importance to the history of the alphabet is very great, but it has been somewhat transformed by a few scholars. I do not want to deal here with this problem in greater detail, as I hope to do so elsewhere.

The ten (2) known early Canaanite inscriptions found in Palestine, mainly in the last 14 years, can be divided into three groups :—

A, Three of them belong probably to the 18-17th centuries B.C. : the Gezer potsherd (FIG. 5, 1), the Shechem stone plaque (FIG. 5, 2), and the Lachish Dagger (FIG. 5, 3).

B, Two or three may belong to the 14th century B.C. : the Tell el-Hesi potsherd (FIG. 5, 4), the Tell el 'Ajjûl pot (FIG. 5, 5), and perhaps the Beth Shemesh Ostrakon (FIG. 5, 6, and FIG. 6).

C, Four Lachish inscriptions (FIG. 7, 1, 2, 4, 5) belong probably to the second half of the 13th century B.C.

Many scholars believe that the writing of these inscriptions 'constitutes an important "missing link" in the history of our own alphabet, representing the long-sought

---

<sup>2</sup> Besides, we may add here the signs painted or engraved on a few stones (v. for example FIG. 7, 3), in the foundation of the temple of Jerusalem, which closely resemble some signs in one of the mentioned Lachish inscriptions.

## THE ORIGINS OF THE ALPHABET

intermediate stage between the Sinaitic and the earliest known Phoenician forms' (Gaster). Have we really sufficient proofs of evidence to be able to affirm that in the Canaanite inscriptions 'we now have a bridge thrown across the gap between the proto-Sinaitic inscriptions and those of the Early Iron Age' (Albright), or, in other words,

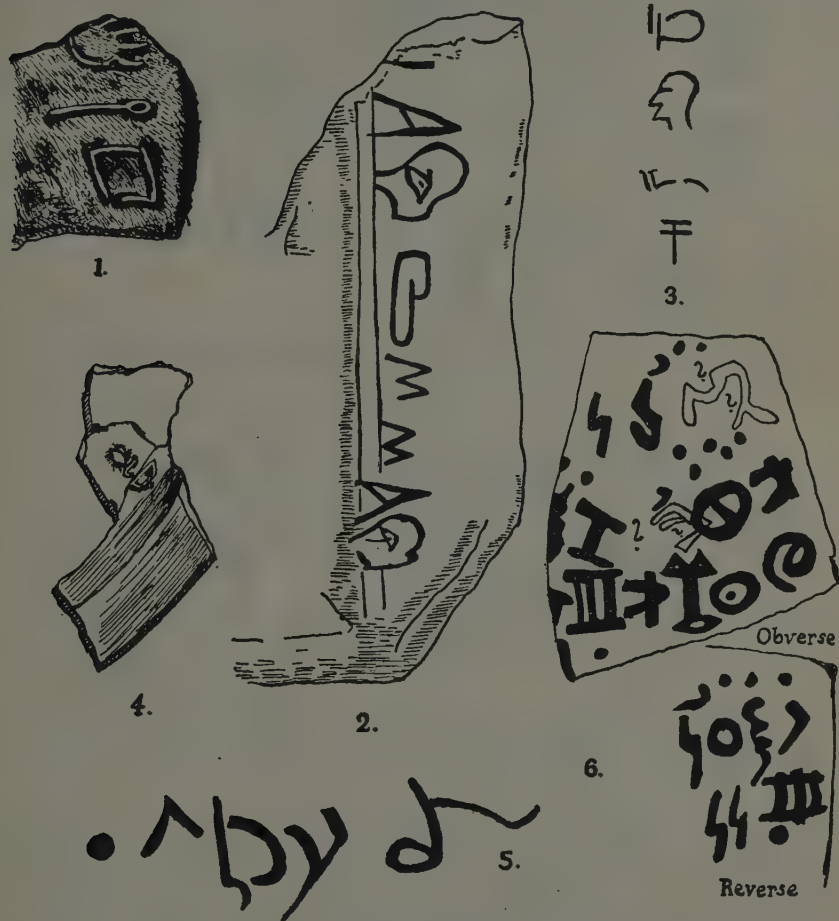


FIG. 5. (1) GEZER POTSHERD (c. XVIII-XVII CENT. B.C.); (2) SHECHEM STONE-PLAQUE (c. XVIII-XVII CENT. B.C.); (3) INSCRIPTION LACHISH IV (DAGGER) (c. 1700-1600 B.C.); (4) TELL EL-HESI POTSHERD (c. XIV CENT. B.C.); (5) TELL EL-'AJJUL INSCRIPTION (c. XIV CENT. B.C.); (6) BETH SHEMESH OSTRACON, ACCORDING TO YEIVIN

that their characters 'obviously stood midway between the much discussed semi-hieroglyphic Sinai script and the later Phoenician alphabet' (Gardiner)?

If we take into due consideration (1) that all the scholars who hold the mentioned opinion, base their identification of the single Canaanite signs on the palaeo-Sinaitic

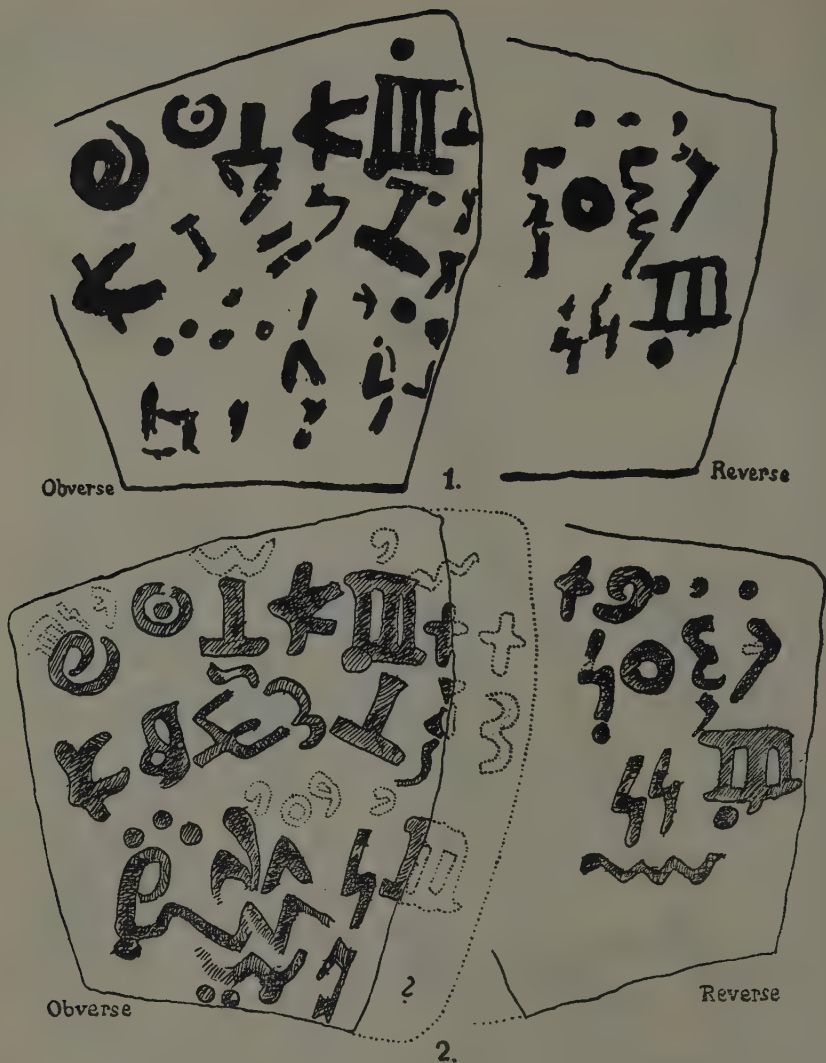


FIG. 6. BETH SHEMESH OSTRACON; (1) ACCORDING TO DUSSAUD; (2) ACCORDING TO GRIMME



## THE ORIGINS OF THE ALPHABET

script on one hand, and on the North-Semitic alphabet on the other, and so easily arrive at the conclusion that the Canaanite inscriptions constitute a missing link between the two systems ; (2) that, nevertheless, in their reading of the single Canaanite signs they differ greatly from each other ; (3) that the very few identifications agreed on by all (or nearly all) scholars can be explained without accepting the theory of the 'missing link' ; (4) that the Canaanite signs do not present generally, even from the purely external point of view, an intermediate stage between the palaeo-Sinaitic signs and the North-Semitic

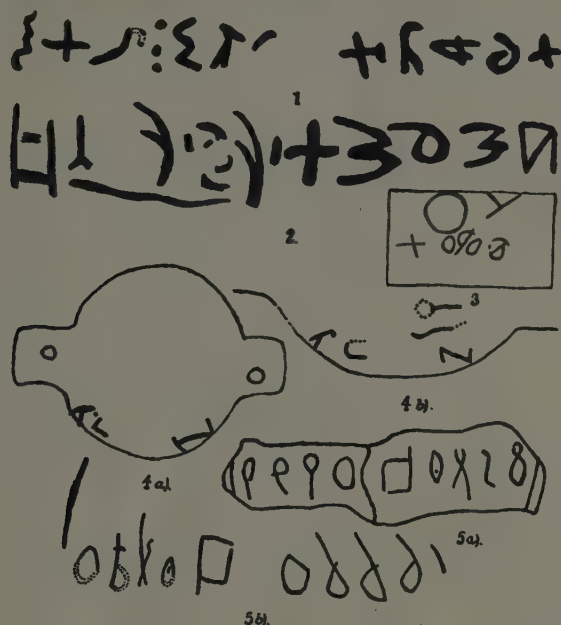


FIG. 7. (1) INSCRIPTION LACHISH I (EWER) (XIII CENT. B.C.); (2) INSCRIPTION LACHISH II (BOWL, XIII CENT. B.C.); (3) INSCRIPTION ON A STONE IN THE FOUNDATION OF THE TEMPLE OF JERUSALEM; (4) LACHISH III (CENSER-LID) (XIII CENT. B.C.); (5) LACHISH III (CENSER-LID) (XIII CENT. B.C.); (a) ACCORDING TO GASTER (b) ACCORDING TO OBERMANN

letters, we must conclude that it is premature to present this opinion as an unquestionable certainty, and that it is only a possible, or a more or less probable hypothesis.

In these circumstances it is preferable to consider the Canaanite inscriptions as another, more or less independent effort (3) of the second millennium B.C. to introduce an alphabetic writing. It does not exclude, of course, the possibility that this attempt is in some way connected with the Egyptian, the palaeo-Sinaitic writing or the Cretan scripts on one hand, and the North-Semitic alphabet on the other. It is true, however, that the writing of the Canaanite inscriptions is much nearer to the North-Semitic alphabet than all the other attempts. Thus, if the agreed decipherment of some of the Canaanite signs be right, it is quite possible that we have in these inscriptions the prototype of our alphabet.

<sup>3</sup> It is even possible that we have in the Canaanite inscriptions more than one attempt.

In default of other proofs of evidence, it is more reasonable to believe that the actual prototype was not remarkably different from the writing of the earliest North-Semitic inscriptions now extant, which are as early as the third group of the Canaanite inscriptions. The North-Semitic alphabet was so constant for many centuries that it is impossible to think that the eventual changes in the first century of its existence were so radical that many characters became quite different from one another.

Until nineteen years ago the native epigraphy of Syria and Palestine was extremely unsatisfactory. The earliest datable known examples of the North-Semitic alphabet were (a) the Moabite stone or stone of Mesha (2 Kings, III, 4-5), dating from about the middle of the 9th cent. B.C.; (b) a Phoenician inscription, found in Cyprus, on the fragments of a bowl dedicated to Ba'al of Lebanon, probably of the same century; (c) some Aramaic inscriptions (from Zenjirli) of the 9th and 8th cent. B.C. These inscriptions and particularly the first one constituted the starting-point for the study of the alphabet.

A new chapter in the history of writing was begun with the discovery, by P. Montet, in 1923 at Byblos, of the Ahiram epitaph. About its date there has been some disagreement. While several scholars prefer the 10th, 11th or 12th century B.C., others (and I among them) believe that the only evidence we have is the archaeological one, and this indicates the 13th century B.C. However, the inscription on Ahiram's sarcophagus (FIG. 8, 1) and the graffito on his tomb (FIG. 8, 2) are the oldest North-Semitic inscriptions, followed by the Yehimlik inscription (12th century B.C.), the Gezer calendar (c. 11th century B.C.), the Roueisseh spearhead inscription (c. 11-10th century B.C.), the Abiba'al (FIG. 8, 3) and Eliba'al inscriptions (10th century B.C.).

According to my opinion only these inscriptions can be considered as a trustworthy basis for a positive opinion. Thus, the incontestable facts about the original alphabetic writing may be summarized in this way: the earliest stage (13-10th century B.C.) of its history, the North-Semitic alphabet was used by the Semitic speaking inhabitants of Syria and Palestine, and was quite familiar to them. This script, compared with that of the Phoenician and the early Hebrew inscriptions of the first half of the first millennium B.C., shows, as already mentioned, close resemblances even in detail. This is the best evidence that the form of the original letters was constant, and would not differ widely from their later form.

As the characters of the Ahiram inscriptions now show a certain external evolution we can assume that the North-Semitic alphabet was at least one or two centuries older than the 13th century B.C. On the other hand, cuneiform writing was currently used by the Semites of Syria and Palestine at the date of the Tell Amarna letters (15-14th century B.C.). This may be evidence that the alphabet was still of recent origin, although it is quite possible that side by side with the cuneiform script, used for diplomatic purposes and for international business, there existed already a common native script. Consequently, we can date the origin of the North-Semitic alphabet at the end of the first half of the second millennium B.C. The nationality of its inventors is unknown. The clue given by the significance of the traditional names of the letters is too slight: the eventually Aramaic form of these names in Greek is not decisive evidence.

Canaanite Palestine, the geographical centre of the greater Egypto-Mesopotamian civilization, or the 'Syrian littoral with its highly developed culture in the second millennium B.C., and a well-organized and active priestly literary school, might conceivably have been the centre from which strong influences along this line emanated' (J. W. Flight). There was always an active movement of cultural elements tending to create an





almost imperceptible synthesis. Syro-Palestine received elements of culture from every surrounding region ; from Egypt in the southwest, Mesopotamia in the northeast, Anatolia in the north, from the islands of Crete and Cyprus and later on from Greece in the west and handed those elements on, somewhat altered as a rule, to other contiguous regions.

It is not in Sinai, the mountain desert region, that the origin of our alphabet is to be sought : Dr Yeivin is certainly right when he points out in his criticism of my theory, that many prophets were born in little towns far away from international commercial routes or in desert villages, etc., but he seems, for example, to have forgotten the difference between the divine and philosophical thoughts of the prophets and the extremely practical purposes of the alphabet. At any rate, it is quite evident that Palestine and Syria offered all the required conditions to bring about the invention and the elaboration of alphabetic writing.

This hypothesis leaves sufficient room for the influence of the older systems : the Egyptian, the cuneiform, the Cretan, and perhaps also the prehistoric geometric signs. It is unlikely that the inventors followed no precedent, and it is extremely improbable that an alphabet invented in Syro-Palestine in the second millennium B.C., should be uninfluenced by the scripts of Egypt or Babylonia, or Crete. Both the conception of consonantal writing and, perhaps, if it exists in the original North-Semitic alphabet, the acrophonic principle, may have been borrowed from Egypt. The influence of the Babylonian writing may be in the names of some letters. The influence of the Cretan scripts and of the prehistoric geometric signs, may be purely external (the form of some letters), but the alphabetic signs may have originated in conventional signs ; it is to be supposed that they were mainly arbitrary inventions.

At any rate, it must be said that the great achievement of the invention was not the creation of the *signs*. It lies in the adoption of a purely alphabetic system, which, moreover, denoted each sound by one sign only. For this achievement, simple as it *now* seems to us, the inventors are to be ranked among the greatest benefactors of mankind. No other people in the world has been able to develop a true alphabetic writing. The highly civilized peoples of Egypt, Mesopotamia, Crete, Asia Minor, China, Central America, etc., reached an advanced stage in the history of writing, but could not get beyond the transitional stage. A few peoples (the Cypriotes, the Japanese and others), developed a syllabary. But only the Syro-Palestinian Semitic people produced a genius who invented the alphabetic writing, from which have descended all past and present alphabets. Each civilization would modify its script and time might make its relation to some of its near relatives quite unrecognizable. Thus, the Devānagari syllabary, the great mother-script of India, the Corean Alphabet, the Mongolian scripts, etc., are derived from the same source as the Greek, the Latin, the Runic, the Hebrew, the Arabic, the Russian alphabets, although it is practically impossible for a layman to see a real resemblance between them.

The main characteristics of the North-Semitic alphabet are that it consisted of 22 letters or symbols, which correspond roughly to the first 22 letters of its descendant, the Greek alphabet. The method of writing was uniformly from right to left. The 22 letters expressed consonants only, though some of them came to be used as vowels. This absence of vowels has not been satisfactorily explained. It has been conjectured that each letter at first represented not a single sound but had a syllabic value. The supposition is used in support of the hypothesis of the Egyptian origin and is suggested by it. Another explanation is that the vowels were supplied locally, the sound given to

## THE ORIGINS OF THE ALPHABET

them varying with the different dialects, so that the inventors left the vowels to be supplied according to local practice. This is hardly conclusive however. At any rate, we must take into consideration the fact that the alphabet was created for the Semitic languages and is sufficiently suited to them; this is also proved by the fact that even nowadays neither the Hebrew nor Arabic language use the vocalic punctuation except in a few justifiable cases. It is connected with the peculiarity of the Semitic languages, based chiefly on the roots (*i.e.* the fundamental conception), which are consonants, while the vowels give us only the complements, the details (the part of speech, the voice, the mood, the tense, etc.).

Some scholars believe that, as the North-Semitic alphabet did not possess vowels, it cannot be considered as a true alphabet; according to them, only the Greeks created an alphabetic writing. This opinion is a great mistake. The North-Semitic script was from the first moment of its existence a true alphabet. It was not perfect. But perfection has not yet been reached by any alphabet, although this end does not perhaps seem very difficult of achievement. A perfect alphabet would represent, as mentioned above, each sound by a single symbol, and not more than one sound by the same symbol. As it is, all alphabets omit symbols for some sounds (representing these, when necessary, by combinations of other symbols), while most of them contain redundant letters. It is probable that writing was in the first place an attempt to represent speech, but even in those early stages the attempt was largely a failure. The number of letters was too small in the beginning and they have never been sufficiently increased, while the phonetic system of any language is far too complicated to be expressed in writing by any reasonably small group of symbols. However, in the long history of our alphabet, it is fairly easy to attach a constant permanent value to the various consonantal sounds, but it is quite different with the vowel sounds. Today the same vowel indicates varying sounds, and it is almost impossible for us to know what exact sound was given to it by ancient peoples. This difficulty will be appreciated more fully if we reflect that in England, for example, the same word is pronounced very differently in different parts of the country, and it is due rather to the varied methods of pronouncing the vowels than to those of pronouncing the consonants. These remarks will explain better why the original North-Semitic purely consonantal alphabet could remain almost unaltered for so many centuries.

A few words about the names of the letters: the value of each consonant is the value of the first letter of its name: *b* of *beth*, *g* of *gimel*, etc. The names (*aleph*, *beth*, *gimel*, etc.) may have been derived from the sign, and not the sign from the name, although the symbol was intended to represent the initial consonant of the name only.

Some of the letters may be regarded as additions since they appear to be differentiations of letters with related sounds. It is noteworthy that the names of these new letters are the most difficult to interpret and have not been explained satisfactorily. The other names ('house', 'door', 'camel', 'hand', 'eye', etc.) are very easy to explain.

The Hebrew order of the letters seems to be the oldest. The order of the letters follows the acrostics in Lamentations, 1-4, Proverbs 31, 10-31, Psalms 25 (the *qoph* is missing), 34, 111, 112, etc. In the excavations of the Wellcome-Marston Archaeological Expedition at Lachish, a schoolboy's scribbling including the scratching of the first five letters of the early Hebrew alphabet in their conventional order has been found on the vertical face of the upper step of the staircase which led up to the Palace. 'It is the first example of the Hebrew alphabet being learnt systematically' (Inge): it belongs at least to the 6th century B.C.

There is in the order of the letters of the North-Semitic alphabet some appearance

## ANTIQUITY

of phonetic grouping, but this may be accidental. The meaning of the names seems to affect the arrangement. Both the names and the sounds rest mainly on tradition.

The most ancient transliterations of the Hebrew letters into their Greek equivalents, and comparison with other Semitic languages, show that the early distinctions of the North-Semitic alphabet between some letters (for example between *samek* and *sin*) have been lost at a later stage. Some scholars attribute this fact to the use of Aramaic, in which for example, *samek* displaced *sin*, etc.

We conclude with a few words about the recent researches on the origins of the alphabet. Analogously to what has been rightly said about Syro-Palestinian archaeology as a whole by one of the most eminent Orientalists (Albright), it must be said especially that one of the most curious aspects of the researches in what American scholars have begun to call 'alphabetology' (that is to say in the field of the origin and the early history of the alphabet), 'is the speed with which old problems are being solved, and new ones introduced into the foreground'. It is to be hoped that the next substitution of new problems for old, will be more constant. The last twenty-five years of excavations and researches have enormously increased our knowledge of the subject with the result that each new epigraphic discovery has been claimed as providing the missing link. And yet 'many aspects and stages of the process are still in that twilight zone between conjecture and certainty', so that the general picture is still very far from being neat. As it is, we must be extremely cautious in our theorizing; a single new discovery may compel us to alter completely an opinion considered hitherto beyond doubt. And in these as in other researches, what is particularly needed is absolute objectivity: this was hardly the case in the pre-war days of exaggerated nationalistic fervour.



# Did Hengist settle in Kent?

by E. G. M. FLETCHER

THE article by Mr J. N. L. Myres in *ANTIQUITY* (December 1942), discussing the earlier articles by Mr K. D. M. Dauncey (March 1942), and by Mr Dayrell Reed (June 1942), has revived, among other matters, the vexed problem whether the Hengist and Vortigern story is true, and whether the earliest settlement of Saxon invaders in this country was in Kent or elsewhere.

Mr Dauncey claimed that the evidence from the cremation cemeteries of Norfolk and Lincolnshire indicated a primary military settlement in those areas. Mr Dayrell Reed, while rejecting the specific conclusions of Mr Dauncey, proceeded to assert, largely on the strength of citations from medieval writers, that the cremation cemeteries in question were evidence that Hengist's mercenary army was established by Vortigern in that region to fight against the Picts shortly after the year A.D. 446.

Mr Myres, in reviewing what is known of Saxon cremation cemeteries, exposes some fallacies in the assumptions made both by Mr Dauncey and by Mr Dayrell Reed, and stresses the inadequacy of the archaeological material from which their conclusions were drawn. It has to be remembered that in the present state of the evidence it is still open to speculation : (a) what is the chronological priority as between cremation and inhumation as an Anglo-Saxon burial-rite in England ; (b) whether an original body of federate mercenary Saxons would be more likely to adhere to their barbaric rite of cremation, or because of their pacific association with Romano-British conditions would be readier to adopt the more cultured native rite at an earlier date than in the case of an immigrant body intent on conquest and settlement, and also (c) whether there is any valid distinction between ' military ' and ' civilian ' settlement in the earliest period of the Conquest.

The outstanding fact to which attention is drawn by the series of articles is that the Roman walled towns of Caistor-by-Norwich, Ancaster, Lincoln, York, Malton and Cambridge are ' all marked on the Anglo-Saxon map by the presence of one or more cemeteries in which cremation, if not the sole rite, is at any rate substantially employed '. This association appears to distinguish the Roman towns of this traditionally Anglian area from those of Saxon areas or of the southeast.

Mr Myres suggests that it would be wise to forget the fascinating stories of Mr Dauncey and Mr Dayrell Reed until we are better able to date the cremation pottery found in Anglo-Saxon cemeteries. But, in the meantime, perhaps it is not unprofitable to re-examine the exact weight to be given to the earliest written authorities—Gildas, Bede, Nennius, and the Chronicle. One is tempted to do so because Mr Myres confesses his attraction to the hypothesis that Hengist and his mercenaries were established in this Anglian area, and his reluctance to accept it entirely appears chiefly due to an unwillingness to doubt the traditional story of Hengist's settlement in Kent.

The question therefore arises : how far is one induced to assume, on the basis of the written authorities with their varying degrees of credibility, that the *original* settlement of the *foederati* was in Kent ? Can one fairly invoke the testimony of Bede as an authority for the Hengist in Kent tradition ? Is it a ' desperate remedy ' to ' explain everything by saying that Hengist and his people never did settle in Kent and that in this matter we have all been misled for 1200 years by the Venerable Bede ' ?

Gildas did not profess to be writing precise history, but there is no reason to doubt his general account of the British invitation to the two Saxon leaders into the island to

repel the northern nations. With this object the *foederati* were first settled in 'the eastern part of the island' (*in orientali parte insulae*). This phrase would seem to apply equally to a settlement in Kent, Norfolk, Lincoln or Deira. This may indeed have been the extent of Gildas' knowledge, but if he had intended to refer to Kent one might question whether 'the eastern part of the island' would be the most apt expression to use. Gildas does, however, state (c. 23) that a considerable time (*multo tempore*) elapsed between the date of the arrival of the Saxons as mercenaries, though making more and more extortionate demands, and the date when they eventually carried out their threats to break the Treaty and plunder the whole island. After the rupture of relations there was chaos, plunder and destruction, until after a further interval of time (*aliquanto tempore*) the Britons rallied under the leadership of Ambrosius Aurelianus.

On general grounds one might have assumed that the most appropriate base for a mercenary army to be used against the Picts would have been considerably further north than the Isle of Thanet, and if the authority of Gildas stood alone—and it is after all the only nearly contemporary record—there was a substantial interval of time between the treaty occupation of Hengist (assuming for this purpose that the occupation was elsewhere than in Kent) and the subsequent Saxon occupation and conquest of Kent.

In *Roman Britain and the English Settlements* (p. 378), after referring to the Treaty settlement which underlay the first occupation by the Saxons, Mr Myres says that 'all subsequent writers from Bede and Nennius onwards have placed the scene of these events in Kent'. Nennius admittedly does so, but does Bede? Unless confirmed by a better authority such as Bede, or by archaeological or other evidence, one would hesitate to rely on the testimony of Nennius, in the *Historia Brittonum*. In its most graphic and romantic sections it quite unambiguously ascribes the settlement of Hengist and his followers first to the Isle of Thanet and then to the rest of Kent, with a final extension to Essex, Sussex and Middlesex (chs. 37 and 46). Mr Dayrell Reed, indeed, cites Nennius in support of his own argument for finding Hengist in East Anglia, but, by a curious translation, he fails to make the most of his citation. Mr Dayrell Reed writes: 'Nennius, in his *Historia Brittonum*, ch. 50, asserts that Aesc after the death of his father Hengist (488) returned to Kent and assumed the Crown from the sinistral parts of the island'. Incidentally, the quotation is not from ch. 50 but from 'Arthuriana' or ch. 56 (according to the Mommsen edition in *Monumenta Germaniae Historica*), and the passage is: '*Mortuo autem Hengisto Oetha filius eius transivit de sinistrali parte Britanniae ad regnum Cantorum et de ipso orti sunt reges Cantorum*'. Now, 'transivit' does not signify 'returned'; Nennius in this particular passage merely asserts that Aesc crossed into Kent after the death of Hengist. It is not a necessary inference from this passage that Aesc had previously been established in Kent or that he went to Kent to 'assume the Crown'. Moreover, it is Aesc and not Hengist who is here indicated as the founder of the Saxon dynasty in Kent.

While there can be no doubt that the Hengist in Kent tradition has throughout the centuries been derived from Nennius, and to some extent supported by entries in the Anglo-Saxon Chronicle, can a modern historian also claim that the tradition has the further support of the Venerable Bede?

The account in Bede (*Hist. Eccles.*, I, 15) is based primarily on the text of Gildas. Although Bede appears never himself to have been in Kent he was, as has been pointed out, well documented as to Kentish history by ecclesiastical friends at Canterbury. Bede repeats the phrase of Gildas about the *foederati* having a place assigned to them to live in 'in the eastern part of the island'. He does not elaborate or particularize, and surely there is significance in his omission to do so, or to make any mention of the original

## DID HENGIST SETTLE IN KENT?

settlement being in Thanet or in Kent. Bede lends no support to the circumstantial romance contained in Nennius with its more detailed geographical details.

After interpolating the famous passage as to the Continental derivation of the Saxons, Angles and Jutes, Bede then adds some information which is not derived from Gildas and which is presumably regarded as the authority for suggesting that Bede supports the tradition of Hengist in Kent. The passage is: 'The two first commanders are said to have been Hengist and Horsa, of whom Horsa, being afterwards slain in battle by the Britons, was buried in the eastern parts of Kent where a monument, bearing his name, is still in existence'.

It has often been noticed that Bede's reference to the names even of the Saxon chieftains is a guarded one; he is explicitly recording tradition, and not historical fact. The statement that Horsa was slain in battle with the Britons and buried in the eastern parts of Kent is necessarily bound up with the tradition that Hengist and Horsa were the names of real heroes. If authentic it would indicate, not necessarily that the original settlement of the *foederati* was in Eastern Kent, but that the Britons were strong enough after the Treaty relations had been broken off to contest the occupation of that territory. This is contrary to the general impression given by Gildas. It would seem that in Bede's day the tradition of Horsa's death in eastern Kent was due to, or at any rate associated with, the belief that a monument bearing his name had been erected there to his memory. On all general grounds we must feel very sceptical about the erection of any such monument.

It follows that on a critical examination of the text the authority of Bede can hardly be used to support the tradition that Hengist and his people made their original settlement in Kent. Bede adds substantially nothing to Gildas on the question of where the *foederati* were originally settled. It may have been Kent or elsewhere on the East Coast. Bede supplements Gildas by giving the traditional names of the Saxon heroes, but he makes no reference to the original association with Kent recorded in the *Historia Brittonum*.

There remain the entries of the Anglo-Saxon Chronicle for the years 449, 455 and 457. This is at best a ninth-century document which from internal evidence cannot pretend to historical accuracy for fifth-century events. The topographical details have long been a subject of mystery. In the present connexion it is sufficient to point out that if Aylesford and Crayford were the sites of battles in Kent shortly after the arrival of Hengist, then the Chronicle version of a conquest of Kent by Hengist and his troops is inconsistent with the story described by Gildas of a pacific Saxon occupation of Kent.

The entries in the Chronicle are consistent with the theory that there was a Treaty settlement of Hengist and his original folk elsewhere than in Kent, followed after an interval of eight years or more by a campaign against the Britons throughout the breadth of the island, as a result of which the conquerors established a Kingdom of Kent under Hengist, as well as other permanent settlements in east Anglia, and the Midland areas of penetration.

Bearing in mind the trend of archaeological evidence for priority of Saxon occupation being elsewhere than in Kent or the Thames region (e.g. Baldwin Brown, *The Arts in Early England*, IV, 593; E. T. Leeds, *Antiquaries Journal*, 1933, XIII; Mortimer Wheeler, *London and the Saxons*) one ventures to suggest, that pending the results of further archaeological investigation the oldest written authorities do not themselves warrant any strong *a priori* assumption that the mercenaries invited to our shores by King Vortigern were given any part of Kent either for purposes of settlement or as a base for military operations. On this geographical question Gildas is silent, Bede is guarded, Nennius romantic, and the Anglo-Saxon Chronicle so untrustworthy as to be almost worthless.



## Avebury

The great importance and interest of the acquisition of the Avebury site by the National Trust deserves the widest notice and with the kind consent of the Editor of *The Times* we are able to reproduce the article which was published on 23 March last. It will be remembered that Mr Alexander Keiller wrote two reports of his work at Avebury in *ANTIQUITY* (December 1936, and June 1939) with numerous illustrations. A few copies still remain ; applications (enclosing 10s.) should be sent to 24 Parkend Road, Gloucester.

**T**HE National Trust has acquired 950 acres of land at Avebury for the nation. The purchase includes the greater part of the group of prehistoric remains that make this one of the most important archaeological sites of Europe.

Some 300 acres have been bought from Mr Alexander Keiller, F.S.A., whose work and discoveries at Avebury since 1925, carried out at his own expense, have added distinguished pages to the story of British archaeology. Within the 300 acres are all but a small part of the Avebury Circles and the immense bank and ditch that surround them ; the northern third of the West Kennet Avenue of megaliths running south from the Circles ; and the Neolithic site of Windmill Hill, a mile and a half northwest of Avebury. The Trust has at the same time bought Manor Farm, of 650 acres. The other part of the Avebury Circles is on Manor Farm, which extends southwards on both sides of the West Kennet Avenue towards the Bath Road.

The museum and most of the present-day village of Avebury are comprised in the purchase from Mr Keiller, which covers all his Avebury property except the sixteenth-century manor house and some adjacent land that does not include any known sites of prehistoric interest. The National Trust, in its announcement of the transfer, pays a tribute to the generous way in which Mr Keiller has helped to secure this noteworthy addition to our national treasures.

A public appeal for funds to meet the cost was considered impossible in war-time. Happily the Pilgrim Trust and Mr I. D. Margary, F.S.A., came forward with gifts that covered the purchase of Mr Keiller's property and went some way towards buying Manor Farm. The acquisitions should facilitate the completion of the Wiltshire County Council's planning scheme for the preservation of the Avebury Countryside from undesirable development. The financing of that scheme was among the objects of an appeal made in 1937 by a number of societies, among them the National Trust, which hopes to have recourse now to the preservation fund for a grant towards the purchase of Manor Farm, leaving the balance to be raised by mortgage.

The great monument of Avebury was built over 3,500 years ago, in the first phase of the Early Bronze Age. With its outer ring of massive stones enclosing over 28 acres, and its bank, three-quarters of a mile in circumference, rising originally 50 ft. above the bottom of the ditch, it is the largest and most impressive work of its kind in all Europe. Here prehistoric man, it has been written, put forth his mightiest effort. Yet for centuries this revealing heritage was allowed to decay, when not actively ill-used. The megaliths were treated as quarries for building-stone ; in the Middle Ages many were buried. Heedless behaviour is not unknown at the present day : in the past month two megaliths have been newly defaced with a bench-mark of the Ordnance Survey, cut deep with hammer and chisel.

Nevertheless, the general picture is to-day far more cheering than for a long time past. For that we owe thanks to Mr Keiller and the Morven Institute of Archaeological Research, of which he is the director, for their work of preservation and restoration. The institute is named after Mr Keiller's estate in Aberdeenshire, inherited from his father while he was still a child. The work he and his colleagues have done at Avebury has been carried out in co-operation with the Ministry of Works, but the whole cost has been met by Mr Keiller from his own resources. That it has been a heavy cost is evident from the labour alone involved—from 40 or 50 to 100 men at a time have been employed during the excavations—apart from the work of a staff of trained archaeological specialists.

Mr Keiller bought Windmill Hill, his first acquisition in the district in 1925 and 1926, and from then till 1929 he and his staff excavated the site. In their view Windmill Hill matters even more than Avebury itself, for excavation has shown it to be the type habitation site for the period which Mr Keiller has termed ' Neolithic A ' in western Europe. In 1936 the Windmill Hill ditches examined earlier were re-excavated and turfed on the virgin chalk, restoring their original structure. In 1934 and 1935 the Morven Institute worked on the West Kennet Avenue, and throughout 1937, 1938, and 1939 on the Avebury circles. The completion of these Avebury excavations was scheduled to occupy another 10 years had not the war interrupted them. In 1938 the Avebury Museum was opened in the eighteenth-century stables of the Manor House.

In these years illuminating discoveries have resulted, not to speak of much tidying-up and removal of obstructions. The fortunate circumstance that the chalk retains indefinitely evidence of any human disturbance has made it possible to determine exactly where each megalith stood and, if it survives, to set it up as it was originally. Not only that : there still remain the impressions left by timber baulks and stakes used in erecting the stones, to show how the builders worked. Broken megaliths have been reconstituted by making plaster casts of all fractured faces and juggling with them till they fitted together. The holes once filled by stones since destroyed or removed are now marked by small concrete pillars. Parts of the great ditch have been cleared of refuse that in places almost filled them.

Here is a summary of what has so far been achieved in restoring the megaliths :— In the northern third of the West Kennet Avenue, before work began, three stones were standing and nine were fallen ; another, re-erected wrongly at an earlier date, had to be taken down and put up correctly. After the excavations 28 stones stood erect, and all but one of the remaining stone-holes had been identified. The excavation of the western half of the outer circle of Avebury has been completed, as well as part of the southern interior setting, and more than half the original number of stones up to this point now stand erect. Before the excavations here seven stones were standing and ten fallen. To-day 29 stand in their original places, not counting another six stones of the so-called ' Z feature ' ; and all stone-holes of missing stones have been identified.

## Notes and News

### EGYPTIAN BRONZE-MAKING

Some years ago Mr Lucas pointed out that bronze could not in the beginning have been made by melting together a mixture of metallic copper and metallic tin. At that time it was probably done by the smelting of a natural mixture of the copper ore and the tin ore (1), the resulting bronze being accidental, and the workmen having no idea as to why their product was different from that of others.

As the present writer has pointed out (2), there is every probability of such a natural mixture of ores being found in the beds of the two rivers of Byblos (Gebeil) on the Syrian coast—the Phaedrus and Adonis. For both of these rivers flow through a region containing beds of the two ores. Hence it is important to remember that Byblos was the port of all others with which the Egyptians traded throughout their history, and that this trade goes back even as early as the middle Predynastic and the Protodynastic ages, c. 3000 B.C. and earlier. Professor Battiscombe Gunn suggests to me that this occurrence of the necessary natural mixture at the Syrian port of Byblos was the origin of the well-known expression *bia štt* ‘Asiatic copper’. If so, ‘Asiatic copper’ would be the original name for ‘bronze’. Written in a manner slightly different from the later forms the expression ‘Asiatic copper’ occurs in an inscription of the very end of the Sixth Dynasty or later, c. 2450 B.C. (3), and bronze was already by that time beginning to be used occasionally in Egypt (4).

After long ages of smelting the mixture provided by nature, it would no doubt have been realized, as Lucas suggests, that it was the presence of this other mineral that caused the difference between the ‘copper’ from Asia and other copper. This would have led to the next step in the evolution of the industry—the purposeful mixing together and smelting of the two ores. The final step was of course the mixing and *melting* together of the ready-made metals.

This last step could not have been taken before the ability had been acquired of smelting the metallic tin from its ore, and tin had evidently not been smelted before some time in the 18th Dynasty. This is the date to which belong the earliest objects made of that metal (5). Therefore the final step of mixing together the metals could not have been taken before some time in the 18th Dynasty, i.e. at some time within the period 1580–1350 B.C.

It is now possible to define more closely the moment at which this last step had been taken, and metallic tin and copper had been melted together in the crucible. The accompanying drawing dates to the latter part of the reign of Amenhotep III, or in other words to the period c. 1385 to c. 1370 B.C. (6). Hence the date in question must fall at

<sup>1</sup> A. Lucas, *Ancient Egyptian Materials and Industries*, p. 176.

<sup>2</sup> Wainwright in *Journal of Egyptian Archaeology*, xx, pp. 29–32.

<sup>3</sup> K. Sethe, *Urkunden des Alten Reichs*, I, p. 294, l. 11. The inscription is a decree by a successor of Nefer-ka-rê Pepi II, who was the last king of any importance of that dynasty.

<sup>4</sup> Lucas, *op. cit.* p. 177.

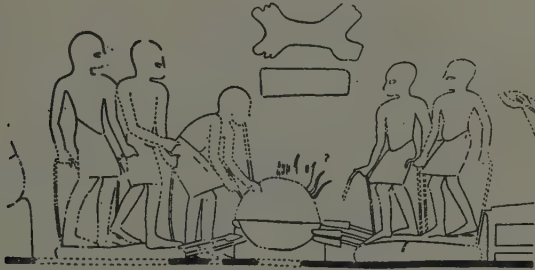
<sup>5</sup> *op. cit.* p. 209.

<sup>6</sup> N. de G. Davies and others, *The Tomb of Two Sculptors at Thebes*, pl. XI bottom register and p. 63. For the date see p. 19.



some time between 1580 and 1370 B.C. The scene under consideration occurs among those of the metal-workers, and shows something being melted in a fire which is forced by four pairs of bellows. Two ingots are figured with the scene, which clearly show that the operation was one of making bronze by fusing together metallic copper and metallic tin, and not by smelting a mixture of the ores of these metals.

On the upper ingot the jagged places on the outline only represent the present day chipped condition of the paint, and are, therefore, to be ignored. The ingot itself is of the hollow-sided shape which was the standard one for copper ingots throughout the Levant (7). It is drawn on this occasion in the somewhat exaggerated shape which the ingot has assumed at the famous native copper-workings in the Katanga district of the



BRONZE-MAKING, EGYPT

Belgian Congo (8). Scheil is therefore correct in colouring it red in his sketches of the painting (9), and Davies in speaking of it as 'copper'. The other ingot, however, Davies calls 'lead', which cannot be correct, for though a copper-lead compound was made in

<sup>7</sup> J. Déchelette, *Manuel d'archéologie*, II, 397-400, fig. 160. Much of the information, but in less detail, will be found in Sir A. J. Evans, *The Palace of Minos* II, 624, fig. 391; IV, 652, fig. 636, where two of the nineteen found at Hagia Triada in Crete are shown. Evans quotes others that have been found at other places in Crete and a piece of one that was found at Knossos itself. Yet others come from Sardinia, Cyprus, Mycenae, Chalkis, and other places on the mainland of Greece. His figure 637 shows how often these ingots are recorded on the tablets from Knossos. They are also brought to Egypt as tribute by the Syrians and Keftiuans, N. de G. Davies, *Bull. Metrop. Mus. of Art*, New York, 1926, *The Egyptian Expedition*, 1924-5, p. 48, fig. 5; Wreszinski, *Atlas zur Altaegyptischen Kulturgeschichte*, pls. 334-5; Nina de G. Davies, *The Tomb of Huy*, pl. XIX top register. For the red colour of Wreszinski's and Mrs Davies' examples see J. G. Wilkinson, *The Manners and Customs of the Ancient Egyptians* (ed. Birch), I, pls. II A, B, following p. 38; Lepsius, *Denkmäler*, III, pl. 116 A. Pl. XXI of Davies' (unpublished) drawings shows that those ingots set down on the ground in Wreszinski's pl. 335, are labelled *hmt* 'copper'.

<sup>8</sup> Beginning of the 16th century A.D., E. Verhulpen, *Baluba et Balubaïsés du Katanga*, p. 98; in the 70's of last century V. L. Cameron, *Across Africa*, p. 239 and fig. (9 or 10 were slung together, and 2 of the slings, one at each end of a pole, constituted a man's load of about 50 to 60 pounds' weight, p. 396); 20th century, Torday and Joyce, *Notes ethnographiques sur des populations habitant les bassins du Kasai et du Kwango oriental*, pp. 51, 52. The present writer possesses one kindly procured for him from the Katanga by Sir Reginald Wingate, and also part of a much heavier one of which only the long sides are hollow. A mould for a cross-shaped ingot very like the one under discussion was found at Zimbabwe, Th. Bent, *The Ruined Cities of Mashonaland* (1893), p. 218.

<sup>9</sup> V. Scheil, *Le tombeau des graveurs*, pl. II, bottom register (published in *Méms. publiés par les membres de la miss. arch. française du Caire*, II, 555-69).

Roman and Coptic days (10), only one of all the Pharaonic bronzes that have been analyzed has been found to contain more than a trace of lead (11). But his nomenclature guarantees the grey or blue colour that Scheil gives to the second ingot. It is evident, therefore, that the second ingot is one of tin.

Thus, this picture of the early 14th century B.C. shows the manufacture of bronze by the advanced method of mixing the metals themselves. It also shows that this step had been taken during the 200 years between 1580 and 1370 B.C. After this it only remained to learn the right proportions of the two metals in order to obtain the best results.

G. A. WAINWRIGHT.

## ARCHAEOLOGICAL OPPORTUNITIES

A correspondent in India has stressed the archaeological opportunity presented when it will be possible to rebuild the bombed areas in our historic towns. The following note supports the necessity for planning excavations on some of the most important sites.

The devastation of parts of some of our older cities and towns, lamentable as it is, will at least give an opportunity to archaeologists such as they have never before received. In large areas of the City of London the modern buildings have been burnt out and since demolished. Of these areas about 100 acres lay within the Roman town and beneath the basements of the buildings which have been destroyed there must lie the remains of the buildings of the Roman town. They lie in London normally below the level of these basements, but the basements of new buildings after the war will be far deeper; they will inevitably destroy the Roman levels. Consequently, if the evidence of these levels is to be recovered, it must be sought for and recorded in the proper archaeological manner before the new buildings are erected or their foundations laid.

The same problem will occur in Canterbury where even less is known of the Roman buildings than in London. Moreover in Canterbury, particularly towards St. Augustine's Abbey, there may well be the remains of an extension of the Roman town in Saxon times. At Exeter also much of the area of the Roman town has been laid waste. Here, however, the Roman level is but a few feet below the modern floors, and the many basements of modern buildings can have left comparatively little intact of the Roman buildings. Already two tessellated pavements have been found there.

The history of Southampton has recently been sketched in these pages. The Saxon town lay away from the medieval and modern nucleus, and what is left of its site should certainly be investigated before rebuilding takes place. Within the medieval town itself there are the sites of several early mansions which should repay excavation, and there is always the chance of finding the remains of wooden houses of the 11th century beneath the floors of the stone houses of the 12th and later centuries.

As far as is known at present this exhausts the list of cities and towns where large scale excavation will be needed. Isolated sites elsewhere, such as that of the Greyfriars in Great Yarmouth and of the Cluniac Abbey at Bermondsey, certainly deserve attention and no doubt will receive it, but the great cities mentioned above propound us the great problem.

To archaeologists the necessity for excavating these sites before rebuilding is obvious. The work must be done then or never, because the new buildings will destroy the ancient levels. To us it is unthinkable that the opportunity will be missed and it is good news that the Ancient Monuments Branch of the Ministry of Works is alive to the situation and is keeping in touch with the planning authorities, both national and local. Much

<sup>10</sup> Petrie, *Arts and Crafts of Ancient Egypt*, p. 103.

<sup>11</sup> Lucas, *op. cit.* p. 426.

will depend upon careful timing, to ensure the investigation first of those areas which are most urgently needed for rebuilding.

But we must always remember that there are those who think differently from us, who do not care for such things and only want as many commercial buildings as possible built in the shortest possible time. Those people can be restrained, but only, in the final event, by public opinion, upon which all official action ultimately depends. We would, therefore, exhort our readers who desire these projects to become realities, to spread the news of them far and wide, to explain to others the necessity for them, and so to assist in forming an instructed public opinion.

### THE FUTURE OF ARCHAEOLOGY

We have been asked to give publicity for a Conference arranged to take place on the 6th-8th August next under the auspices of the University of London Institute of Archaeology. Its purpose is shown in the following statement prepared by Miss Kathleen M. Kenyon, the Acting Director.

A great measure of support has been shown for the suggestion that a Conference on the Future of Archaeology would be of value. A considerable amount of discussion has been going on among groups of archaeologists in the last few months on problems connected with post-war archaeology, and it is felt that a Conference at which these questions could be ventilated would be welcomed. The informal support of archaeologists connected with a large number of different societies has been given to the proposal. It is believed that the Conference will form a useful supplement to the meeting of the Congress of Archaeological Societies which has been summoned to discuss steps to be taken in connexion with the future of archaeology in Britain, but which will provide only comparatively limited scope for discussion.

It has been decided that the geographical range of subjects shall be from Britain to the Middle East, including all the intervening countries in which British interests are concerned. This decision has been reached since a large number of subjects on which discussion is required affect all archaeologists whatever their spheres of work, and since it is felt that anything which will help to prevent archaeologists remaining in water-tight compartments will be to the good.

The subjects provisionally suggested for discussion are:—the Contribution of Archaeology to the Post-war World; the Future of Discovery at Home and Overseas; the Training of Archaeologists; Archaeology as a Career; Planning and the Independence of Societies; Museums and the Public; Archaeology and Education, in Schools and Universities; Archaeology and the State at Home and Overseas. The proposal is that each subject will be introduced by two or three speakers, and that there will then be plenty of opportunity given for discussion.

It is realized that there are difficulties in holding a Conference in wartime, but it is felt that these are more than counterbalanced by the fact that it is essential that plans should be made soon to fit in with general post-war re-organization of cultural and educational matters. It is also necessary to decide whether there are any matters on which the State should be approached, and to provide by a Conference such as this the evidence of a strong body of opinion behind any such approach. It is of course clear that not everyone interested will be able to attend, and that therefore no action could be taken by the Conference which would commit those not present. What is hoped will emerge is views on what action is desirable and suggestions of how an executive body, representative of all interests, to take such action may be created.



It is hoped that Universities and national and local societies will arrange to be formally represented at the Conference. It is also hoped that as many individual archaeologists and members of archaeological societies as possible will attend in order to make the Conference fully representative.

All interested are asked to notify the Secretary, Institute of Archaeology, Inner Circle, Regents Park, London, N.W. 1. A detailed programme will then be sent as soon as possible, and the final application to attend may be made after that. A fee of 7s 6d will be charged for the whole Conference, or Friday, 2s, Saturday and Sunday 4s each.

### THE TARIQ EL-GEMEL AT GERBA, TUNISIA.

In ANTIQUITY, 1938, XII, 479, 480, Mr O. G. S. Crawford gives an account (with plate) of the present state of this causeway which used to join the island of Gerba to the mainland. As he says that he has been unable to find any published account of the construction, I contribute the following to the enquiry.

Writing in A.D. 1526 (1) Leo Africanus mentions the destruction of the bridge by the islanders not very long before then. The island had been reconquered from the Christians by the king of Tunis, 'But after the death of king Hutmen the Islanders returned to their former libertie, and presently broke the bridge from the Island to the maine lande, fearing least they should be inuaded by some land-armie' (2).

On p. 763 in his note 81 to the passage from which this excerpt is taken Brown says that the island was continually being conquered by various people and then rebelling. It was one of these conquerors who is said to have built the causeway, Alfonso v of Aragon, who subdued the island in 1431. However, on the other hand it may have been built by Roger de Loria who conquered the island in 1284 and held it in fief from Peter of Aragon. It was he who built the great fortress called Borg el-Kebir. Thus, whatever its actual date, the Tariq el-Gemel seems to be clearly a medieval construction. The name, however, is modern, for in the account of Alfonso's doings it is merely called el-Kantara 'the Bridge'.

Its medieval date is borne out by the classical accounts of the island under its ancient names Meninx and Lotophagitis, which make no mention of any causeway joining it to the mainland. If there had been one, it is hardly possible that so important a work should not have been mentioned. W. Smith in his *Dictionary of Greek and Roman Geography* gives a good account of the ancient history of the island under its name Meninx, and Pauly-Wissowa in the *Real-Encyclopädie des classischen Altertums-Wissenschaft*, xv, cols. 859, 860, gives a still fuller account, for it is quite a recent work. Many references to literature both ancient and modern are added by each author.

In his *Geography* Ptolemy gives the name Girba as that of one of the towns on the island of Lotophagitis, and Meninx as the name of the other. Thus the modern name of the island, Gerba, goes back to the second century A.D., some 500 years before the Muslim invasion. Hence, it is not Arabic, although it has so very Arabic a look. It may, however, be a Carthaginian name, for that language is Semitic just as is Arabic.

Leo Africanus had good reason to remember the island, for he entitles his section dealing with it 'Of the isle of Gerbi or Zerbi, where Iohn Leo the Author of this Historie was taken by Italian pirates, and carried thence to Rome'. There is little doubt that he was captured in the year 1520 (8).

G. A. WAINWRIGHT.

<sup>1</sup> *The History and Description of Africa*, J. Pory's translation, edited by R. Brown (Hakluyt Soc.), introduction, p. XLVI.

<sup>2</sup> op. cit. III, 735.

<sup>3</sup> op. cit. introduction, XLII.

## Reviews

WHAT HAPPENED IN HISTORY. By PROFESSOR GORDON CHILDE. *Penguin Books, Harmondsworth, Middlesex, England; [300 Fourth Avenue, New York] 1942.* pp. 256. 9d.

The format of this book is no index of its value. In other times it would no doubt have appeared on white, instead of dingy grey, paper, with adequate margins. We must be content with the fact that it has been published at all. It is a book of really first-rate importance, covering the whole of early human history from palaeolithic savagery down to the decline and fall of the ancient world; and it is written in a style both racy and concise. The author is master of a vast mass of knowledge which would overwhelm most people. He sees history through modern eyes and measures it by modern standards, so that it becomes comprehensible to ordinary people. The book sets out to describe how 'Man has progressed during the several hundred thousand years of his existence on the earth', and it is thus 'an extension of the account of Man's progress in the long ages before the dawn of written history, advanced six years ago in *Man Makes Himself*' (Watts & Co., 2s 6d) (1). Chapters II-V are to some extent, as the author states, a recapitulation of 'events and conclusions there set forth'. But they are no mere mechanical condensation, but written with a freshness of touch that keeps them alive and readable.

Perhaps the outstanding merit of the book is that it maintains the same point of view throughout; history and prehistory are seen as a single process of adaptation to environment or of the adjustment of his environment to Man's needs (pp. 7, 8). Man made this possible by the invention of tools; and history thus viewed is largely a study of the improvement of tools and of the various results that ensue therefrom. Now archaeology is for technical reasons mainly concerned with tools and the other concrete relics of the past; it is concerned too with people rather than with kings, (2) whereas history on the whole reverses the emphasis. It is the virtue of the present book that it refuses to lose interest in this process of adaptation and adjustment when it enters the realm of history. This appears in phrases like 'bronze age religion', the constant use of the terms 'bronze age' and 'iron age' to describe 'historical' societies, the emphasis placed upon inventions (or the lack of them, p. 238), the use of mere size (e.g. of towns and populations) as a criterion of progress (p. 154).

The book is, however, no mere primer of 'everyday life in the past'. It is informed by first-hand knowledge, presented with rare and refreshing detachment (3). While never losing sight of the concrete foundations, the author touches on most of the main topics of interest to us nowadays—the economic basis of religion (pp. 88, 190, 192 *et passim*), the true nature of so-called democracy in Athens (p. 185), the inevitable results of a concentration of purchasing power in the hands of a ruling class (pp. 88, 228), the difference between native and foreign exploiters (p. 232), religion and the state (p. 239).

We have already dealt at some length with the earlier part of the story in our previous

---

<sup>1</sup> Summarized and reviewed in *ANTIQUITY* 1936, x, 391-404.

<sup>2</sup> For instance, the Sutton Hoo ship-burial (*ANTIQUITY*, March 1940), tells us at least as much about the arts and crafts of the period, and the technical skill of the artists and craftsmen, as about King Redwald (if it was he). Which is the more valuable 'document'—the grave or the brief account in the O.E. Chronicle?

<sup>3</sup> An extreme example of such detachment is given in the last sentence of the preface.

review. We do not therefore propose to do so again, but rather to refer briefly to the latter portion, and then to deal at random with some striking points. Stated briefly, the ultimate cause of the break-up of earlier civilizations appears as an increase of population, which sets in motion a series of disturbing events. But in fact one should not—and Professor Childe does not—try to fit any one single cause to results that had multiple origins. The important fact that emerges is that, although the Eastern civilizations and their Hellenistic and Roman successors did decline and fall, the cultural capital accumulated was not annihilated. 'Many refinements, noble and beautiful, were swept away. But for the most part these had been designed for and enjoyed by only a small and narrow class. Most achievements that had proved themselves biologically to be progressive, and that had become firmly established on a genuinely popular footing by the participation of wider classes, were conserved, even if temporarily fossilized' (p. 250). Such were many handicrafts, and also husbandry, a money economy (in place of barter), trade (in place of self-sufficiency), writing. Post-Roman Europe, though barbarized, 'did not relapse into the Stone Age, nor even into the La Tène period' (p. 250). In other words progress is a spiral, or a rising graph whose downward curves never reach the previous lows. That conclusion, securely established as it is, should be good medicine for the cynics whose reaction to present troubles is to deny the fact of progress. We fancy that even the blindest of them would admit that he was an improvement upon Piltown Man, to say nothing of the remoter Tarsian lemur.

There would appear to have been four main stages in human history: (1) the stage of savagery, based upon hunting and food-gathering; (2) the stage of barbarism, based upon agriculture and quickly passing into (3) the stage of urban development in which the emphasis is upon trade, away from self-sufficiency; (4) the present stage of power-production, in which direct human and animal labour is being replaced, both in agriculture and industry, by the power derived from natural sources (coal, oil, electricity). Logically the present book should have continued the story down to the 18th century, when the fourth stage began; but we can easily understand that this would have been practically impossible. Nevertheless we hope that the author will not leave the story thus unfinished, and will some day—and soon—complete his already fine achievement by summarizing, from the same admirable point of view, the remaining portion. After all, the materials exist and the subject is at least as germane to an archaeologist as the periods dealt with here in the last four chapters.

We hope that if he does so he will have something to say about the relation between the *size* of a political unit and its economic structure. A reference on p. 231 to 'economically futile political entities' suggests that this aspect has not escaped his notice. Our interest in the problem may be stated in the form of a question—To what extent have modern means of communication (roads and motors, railways, ships, aeroplanes, radio) made obsolete existing political units? It is, for instance, a recorded historical fact that the size of the constituent states of North America was determined mainly by difficulties of travel during the winter. We think everyone will agree that such a factor as this, combined with the natural features of a country, does exercise an influence, to put it at its lowest, in the size of a political unit. Should not, then, such radical changes in means of communication as have occurred in the last hundred years demand equally radical changes in the size of the corresponding political units? And, although we confine the suggestion to size merely, is not the whole political structure of society also involved?

To conclude with a few random comments. P. 43, seed-gathering was still practised in the 18th century by Agows near Lake Tana in Abyssinia (Bruce's *Travels*, 1805,



## REVIEWS

v, 401) and by the Sudanese near Khartum (ib. id. vi, 419, 423); in times of scarcity they made 'a kind of bad bread' from the seed of bent-grasses. P. 67: How did the knowledge of bronze-working reach Mexico and Peru from the Old World? P. 52: The credit for many early discoveries and inventions is attributed to women, 'judged by ethnographic evidence'. Presumably this means that women perform the work in modern primitive societies. But does it necessarily follow that they also invented the processes? Most typewriting today is done by women, but they did not invent the typewriter. P. 88 throws interesting light on the role of the priests and the origin of the class struggle; even before 2500 B.C. wealth was concentrated in the hands of a small class. P. 167: It would be nice to have a series of sketch-maps indicating the expanding and contracting 'continuous areas of civilization'. Could these be given in a new edition?

Pp. 174-175: Communities emerging directly from barbarism into an Iron Age civilization, unfettered by too many legacies from the Bronze Age, were better able to realize the possibilities of certain new discoveries and inventions such as iron tools, alphabetic writing and coined money. This is a most suggestive idea; the process is paralleled by the failure of the most advanced savages of the Old Stone Age, the Magdalenians, who 'were all too successfully specialized for exploiting the Pleistocene environment', to survive a change of environment in competition with 'the humbler groups who had created less specialized and less brilliant cultures further south' (p. 43). Many evolutionary parallels could be cited; has it not also an interest for some of the highly specialized and old-established communities of today?

Pp. 185, 186 (overlapping paragraph). This, too, is interesting to read today. P. 195: The baleful effects of Platonism which still persist. P. 199: Were the foundations of modern descriptive geology really laid by the natural philosophers of Greece? Was there not a long and complete break and a new start? P. 201: Aristotle the fore-runner of Nazism. P. 202: The dependence of the artist on patronage is a neglected aspect in the history of art, but is well exemplified by the new arrangement of the Hermitage Museum at Leningrad. P. 207: Surely Seleucia on the Tigris cannot, as Pliny stated, have contained 600,000 people; it is not big enough. P. 216: The British hill-forts were 'economically just villages' and not towns as has been claimed; we agree. P. 227 contains a historical warning for those who plan their economy in the interests of a governing class. P. 238: 'Imperial Rome made no significant contribution to pure science'.

There is much in the book that throws light on the relations between the religious superstructure and the economic and political basis. Monotheism is seen as 'the ideological counterpart of an international economy' (p. 190). Imperial Rome tolerated no rival of the deified emperor and persecuted Christians for the same reason that Germany does.

We are indebted to the author for a brilliant, stimulating, original and eminently readable book. O.G.S.C.

CLIMATE AND THE ENERGY OF NATIONS. By S. F. MARKHAM. *Oxford University Press*, 1942. pp. 144. 10s 6d.

Climate has often been selected as the determining factor by authors anxious to simplify the complexities of history. In reviving the old tale Mr Markham introduces some significant variations. He has been at some pains to collect conclusions of physiologists and medical men on the human organism's reaction to climatic conditions. I am not qualified to say whether his authorities are well chosen nor even whether his

inferences therefrom are logical. They are that 'the ideal climate is one which, while never or very rarely passing the upper or 76° limit, yet does not fall so low as to demand great efforts to bring indoor temperatures up to 60° or 70° whichever is deemed the most desirable'. Applying this historically, he admits, like Huntington and others, variations in climate during historic times. He even mentions a 1400 year-cycle, but only uses it as a possible explanation for supposed peaks in Egyptian civilization—'beginning' around 4200 B.C. (perhaps the now exploded date for the introduction of the Calendar); 2800, the Pyramid age (dated too high); 1400 'another great period (the 18th Dynasty' already declining by then) and 'a final burst' that 'began under the Ptolemies about a thousand years later'. He quite rightly rejects the totally undocumented assumptions of very drastic climatic changes into which Huntington has been seduced. His original contribution is to insist on the fact that Man can control his temperature and to invoke improvements in the technique of so doing where others were forced to postulate catastrophic climatic change.

Our author contends that 'prior to the invention of the hypocaust system or the fireplace and chimney the ideal conditions would be found in maritime countries with a mean annual temperature of round about 70°. The 70° isotherm is then a good guide as to where conditions for the rise of civilization should be expected, and of course the oldest centres of civilization—Egypt, Sumer and the Indus together with the old Maya Empire—do lie near that line today. Their early supremacy would then be due to their climatic superiority in the absence of efficient temperature control combined with the possibility of supporting 'the man-power essential for the defence of any civilization against races from less favoured areas'.

The transfer of the 'leadership of civilization' from regions along the 70° annual isotherm would then be due to the invention of 'the hypocaust system of heating in which the floors and later the walls of buildings were warmed by the passage of hot air through flues'—an invention attributed to the Lacedaimonians with a reservation in favour of the Ionians in a footnote. 'This discovery and its development naturally gave the wealthy among the Greeks an indoor temperature that could be effectively controlled in all but the hottest months'. 'If we assume that Greece began the hypocaust system about 750 B.C. and that Rome adopted it a little later' we are struck by the same phenomena 'greater prosperity and greatly increased numbers', as followed the adoption of climatic control by western nations in modern history. This is in fact as serious an assumption as the climatic change invoked by Huntington, and its author is bound to adduce some evidence in its support. He gives none. A reference to standard works like Rider's, *The Greek House* and the relevant articles in Pauly-Wissowa will not discover any mention of hypocausts even in baths, let alone in private houses, in Classical Greece. That might be explained away by the negligence of ancient authors and the absence of archaeological evidence. But the well-to-do Hellenistic houses of Delos and Priene mysteriously failed to disclose hypocausts. And today, thanks to the excavations at Olynthus, definite evidence is available for the Classical period too. This flourishing and progressive colony in chilly Macedonia should be expected to have used the most efficient heating systems available. The houses described by Robinson belonged to prosperous citizens. Some had mosaic pavements, many have bathrooms, none hypocausts. The only rooms in which fixed heating appliances—square hearths—were found are plausibly interpreted as kitchens. In the light of this negative evidence any attractive picture of Socrates discoursing at the symposium in a centrally heated house must vanish. The idea that the Greek genius of the 5th century was nurtured by air-conditioning is empty fancy. A climatic explanation that might work for Knossos or Ur, will not work for Athens.

As to Rome we learn that 'the houses of the wealthy had central heating, windows were glazed, and water was generally laid on'. . . . 'The masses of the people could go to the baths in the middle of the Italian winter and there find, for a trifling sum, warmth and entertainment' (the air-conditioned cinemas of U.S.A. are appropriately compared to the *thermae*). But to what period do these remarks apply? In the Oscan period at Pompeii hypocausts had not yet been installed even in the public baths. At Rome they were coming in about 100 B.C. but the multiplication of cheap public baths in Rome and other Italian cities is correctly dated by Mr Markham himself, 'just before the Christian era'. This was hardly the period when Rome began to display the greatest national energy whether in war, diplomacy, or literature. And the popularization of temperature control did not prevent nor arrest the economic, scientific and political decline of which the barbarian invasions were the consequence rather than the cause.

This last fact has not escaped Markham. But he explains the decline of Roman energy by the deterioration of their superb heating systems and baths as a consequence of the Father's uncompromising denunciations of such carnal luxuries, after the conversion of Constantine (a footnote suggests that a shortage of wood fuel may have accelerated the catastrophe). Perhaps in the time of Gibbon, the sole authority cited, the available literary evidence may have suggested that the decline of the Romans' energy became manifest only in the late 4th century. The data subsequently amassed by archaeology leaves no doubt that the decline was all too patent in the 3rd when the energizing hypocausts were still in full blast. The facts of Roman history support the hypothesis no better than do those of Greek.

The intensification of the energy of northwest European nations from which Atlantic civilization sprang is correlated with 'the rediscovery (*sic*) of brick-making, window-glass-making and coal, and the invention of the fire-place and the chimney'. I am not in a position to say whether the dates here given to these events are any more reliable than those just criticized. I am prepared to believe that the outburst of national energy that found artistic and literary expression in the Renaissance was incubated in an atmosphere heated by grate and chimney fires. But before that a no less remarkable, if less spectacular, outburst of energy had been finding expression in technological advances (cast iron for example) that were essential preconditions for improved heating as for the rise of a new science. And as far as temperature control is concerned, I suspect chimneys are much over-rated. Mr Markham fails to appreciate how snug the Eskimo was in his snow house, the Stone Age Orcadians at Skara Brae or the mammoth-hunters at Kostienki.

We may note here a few other passages that seem relevant to the author's competence. On p. 22 Sumer is listed along with Larsa, Susa, Al'Ubaid and Endu (? Eridu) as an 'ancient city'. On p. 46 we are left to infer that Darius was the first to make a canal from the Nile to the Red Sea. On p. 47 Markham writes 'the glory of Greece was founded on a total population of about 300,000 of whom 120,000 were slaves, 20,000 resident aliens, and only about 165,000 free citizens and their families' (apparently confusing Athens and Greece).

It might be unfair to judge the remaining chapters, that are the province of economic geography and climatology, by the standards of scholarship revealed in the historical sections. In chapter VI an attempt is made to devise by an ingenious system of weighting the mean urban temperatures an effective mean temperature for the populations of modern States from giants like U.S.A. and U.S.S.R., to units like Denmark and Esthonia. In VIII the national energy of such States is tested by death rate, infantile mortality, national income and share in world trade. The most energetic nations on these tests enjoy a



climate with means for the coldest month of 30° or more, for the hottest of 75° or less. Death rates, unemployment statistics and the number of wireless sets prove that in England what we archaeologists have been taught to call the Lowland Zone supports the most energetic population. The mental and physical degeneration of 'poor whites' may be traced to 'the lack of heating arrangements, of protection against the sun and of cooling arrangements' in regions like South Africa where climate control is essential. Finally the hope for humanity is to be seen in the development of air-conditioning.

It would be impertinent for a mere archaeologist to criticize these contentions in detail. I must, however, draw attention to two points that enhance my scepticism. From the map on p. 84 the reader would infer that the state-capital, Sacramento, enjoyed the same ideal climate as San Francisco. In reality the modifying effect of the Pacific stops short abruptly at the Coast Ranges and within a hundred miles of the Golden Gate you reach one of the World's hottest spots. Much the same happens in eastern Australia. As the cities on which Markham bases his calculations mostly lie on the coast, I fear they give too attractive a picture, despite all weighting. Secondly Russia's Five Year Plans and the resultant ability to check the full force of the mechanized army of the most highly industrialized country in Europe constitute surely an amazing display of national energy, even though it be not measurable in terms of world trade. But on the climatic standards here adopted Russia and Siberia certainly stand very low indeed and admittedly had made little progress in the sort of temperature control envisaged in the book (though, I repeat, stove heating, apparently descended from neolithic Tripolye models, is more efficient than our author will admit). The book has done a service in calling attention to the potential significance of temperature control, but has inevitably failed to extract from the complexity of History any single dominating thread of causation.

V. GORDON CHILDE.

**EXCAVATIONS AT RAIRH.** By K. N. PURI. Department of Archaeology and Historical Research, Jaipur State, India. 1941. pp. iv, 73, 36 plates.

The work carried out in certain of the independent Indian States in the field of archaeological studies has perhaps tended to be obscured by the more striking achievements of the Archaeological Survey in British India on such sites as Mohenjo-daro and Harappa. The task of conservation and recording by colour photography of the famous Ajanta mural paintings carried out by the Archaeological Department of Hyderabad State within recent years is perhaps most widely known, but if less popular in its appeal, the contribution to Indian medieval archaeology made by Dr Puri in this report is never-the less of really first-class importance, and its publication reflects the highest credit on the Department of which until recently he was Superintendent. An earlier excavation of some considerable importance carried out by this department under Dr Sahni has already been discussed in *ANTIQUITY* (March, 1943).

The site at Rairh, to which attention was first drawn by the accidental discovery of a coin-hoard, was found, as a result of the excavations carried out from 1938-1940, to be that of a small township having, apparently, a specialized industry of iron working, and occupied from the 3rd century B.C. to the end of the 2nd century A.D. Although three periods of occupation were detected, the material culture appeared uniform throughout: the mud houses left practically no remains except for certain curious platforms of parallel lines of burnt bricks which seemed to have served as foundations in some instances. Characteristic of the site were soak-pits lined with large pottery rings. Finds comprising

pottery, iron tools, stone beads and various small objects, and no less than five hoards totalling 3075 silver 'punch-marked' coins (6th to 2nd century B.C.).

Apart from these coin-hoards there is nothing that could be called spectacular about the site, yet both in excavation and publication, it must take a foremost and pioneer place in the study of Indian medieval archaeology. Trained as a pre-historian, Dr Puri has applied the scientific method to such structures as remained identifiable and to the finds, particularly the pottery. The scale-drawings of over 80 types of vessels provide for the first time a corpus which, if only dated within the rather wide limits of 500 years (since all types at Rairh seem to persist throughout the occupations) at least gives a beginning by which early (Maurya-Sunga) wares can be separated from late (Gupta, etc.) types. And for one type we may note the possibility of closer dating: Dr Puri points out that four of the coin-hoards were contained in vessels of the same type (his pl. VIII, 6-9) and he plausibly equates the deposition of these hoards with the 'Time of Troubles' immediately following the collapse of the Mauryan empire in the early part of the 2nd century B.C. The immediate value of this corpus of pottery beyond the immediate neighbourhood of the site was brought home to the writer of this review while visiting the excavations now in progress by the Archaeological Survey on the site of the great medieval city of Ahichchhatra near Bareilly, 250 miles northeast of Rairh as the crow flies. On this site, the occupation of which extends from the 3rd century B.C. to the 8th century A.D., the ceramic parallels with Rairh are no less interesting than the disparities: the common Rairh form of bulbous jar (pl. VIII, no. 5) is at Ahichchhatra characteristic of a defined level and associated in at least one instance with a pottery finial of the type of Rairh (pl. VII, no. 46). The coin-hoard type of vessel mentioned above also occurs at the more northern site, and might prove, on the Rairh evidence, a valuable dating criterion. The Rairh soak-pits lined with pottery rings appear at Ahichchhatra in early Sunga levels, but associated with a distinctive fine grey ware which does not appear in the Jaipur site. It is clear that the inter-relations of these and other sites will, when worked out, provide the framework for an absolute chronology of Indian medieval material which can then be correlated with the vague half-legendary written history which strikes the Western scholar, accustomed to the massive and detailed records from the diplomatic and administrative archives of medieval Europe, as so strangely inadequate.

One point brought out by Dr Puri in his report touches on a very wide issue. In several instances, not only with regard to small finds, but also to the enigmatic brick foundations, he has, while wisely refraining from further comment, noted that their best parallels can be found in the Indus Culture. If a reviewer may be permitted the license, denied to the excavator, of being perhaps a little unwise, it is tempting to enquire what these similarities may mean. The debt of Hinduism to the religious concepts and iconography of the Indus Culture has been admitted since Sir John Marshall's detailed demonstration many years ago, and it is possible that the scientific excavation and study of medieval India may show that the debt was not only spiritual, but material. The culture which, in the centuries immediately before the Christian era, could produce great brick-built cities such as Ahichchhatra, covering an area a mile by a mile and a half, seems hardly likely to be a creation of the hard-riding, hard-drinking barbarians reflected in the Vedic hymns. Was the civilization of the Maurya and Sunga dynasties, the unity of which is being shown no less in its potsherds than in its epigraphic and traditional aspects, 'affiliated', in Arnold Toynbee's sense of the word, to the Indus Culture? Further work along the lines which Dr Puri has so satisfactorily initiated should throw light on this fascinating and fundamental question in the history of civilization in India.

STUART PIGGOTT.

DOUZE ANS DE FOUILLES DANS LA NECROPOLE MEMPHITE. *Par* GUSTAVE JÉQUIER. *The Secretariat, The University, Neuchâtel, Switzerland, 1940.* pp. 166, 48 text-figures. 6 francs.

The excavations among the southern group of monuments at Saqqâra, undertaken by M. Jéquier between 1924 and 1936, have revealed in that group an interest and importance almost as great as those of the northern group, which includes the Step Pyramid and its associated structures.

During his researches M. Jéquier worked almost entirely on his own, with the exception of one or two draughtsmen and of course a quantity of native labour. The result was that everything was done under his own supervision. The volume under review gives an account of his work, divided into the periods of the monuments excavated.

Part I (3rd Dynasty) describes preliminary research on the Rhomboidal or Blunted Pyramid of Dahshûr which the author considers may well have been constructed by Snefru's predecessor Houni (Neferka). Part II (4th Dynasty) is concerned with the Mastabat Faro'un (the Pharaoh's throne) which he concludes was built by Shepseskaf, who followed Mycerinus and was the last king of the 4th Dynasty. This striking tomb-structure, which is rectangular and flat-topped and resembles an enormous mastaba, contains an interior which resembles that of 5th Dynasty pyramids, in having, leading from the approximate centre of the north face, a ramp, at a slope-angle of  $23^{\circ}$ , leading to a vestibule and horizontal passage, the latter blocked by three granite portcullis-slabs. At the end of this passage was an antechamber with a pointed roof, and corridor leading westwards into the sepulchral-chamber. The latter resembles that of the pyramid of Mycerinus in having an arched roof of granite slabs hollowed out on the undersides. The serdab recesses were not on the east but the south of the antechamber. It appears that Shepseskaf's important contemporaries were buried elsewhere and not (as was usual with other monarchs) in the vicinity of their king.

Part III (6th Dynasty) is devoted to M. Jéquier's excavations at the pyramid and temple of Pepi II, and those of his queens Neit, Apout, and Oudjebten. His exploration of the pyramid of Pepi II resulted in the finding of additional portions of the Pyramid texts, some of which are not paralleled by those of any other pyramids. An important part of his research concerned some small pyramids previously regarded as those of queens. The excavations showed this assumption to be gratuitous, inasmuch as the pyramids of Pepi II's queens themselves had each a small pyramid near the southeast angle. Jéquier concluded that they may be 'pyramids of offerings' or votive-pyramids, perhaps for the *ka* of those buried in the major pyramids. The group of 16 beautiful wooden boats (la Flotille de Neit) found near the pyramid of Neit, deserves special mention. Fig. 22 gives an excellent reconstruction of the pyramid-complex of Pepi II and his wives. The magnificent wall-paintings in the funerary-chambers of Shy and Sebekhotep, near the pyramid of Pepi II, are also described.

Part IV (Middle Empire) describes the pyramids and pyramidon of Khenzer and an unknown monarch. The latter is still accessible and can be regarded as the most interesting and spectacular of all the 12th-13th Dynasty pyramids as far as their present condition is concerned. It is situated about one kilometer south of the Mastabat Faro'un. Part V (New Empire) describes certain later finds of no special importance.

More detailed accounts of all these excavations are to be found in the quarto volumes issued by the French Institute of Oriental Archaeology, Cairo, and M. Jéquier's task in the small work under review has been 'to recapitulate the results of these excavations, to classify them, to place them in their historical setting' and to assess their scientific import. The thanks of all archaeologists are due to him for the methodical way in



## REVIEWS

which he has excavated his sites, the insight with which he has distinguished between the important and the unimportant, the attractive way in which he has presented his detailed material so punctually after each season's work, and even more for summarizing it in such a handy form in the present work.

L. V. GRINSELL.

MAN'S UNWRITTEN PAST. By EDITH PLANT. *Oxford University Press*, 1942. pp. vi, 262 with 14 plates. 5s. In the series *Realms of Natural Science*.

In this book Mrs Plant has supplied a real need, in that she has collected into one volume many of the results arrived at in the latest publications on Prehistoric Archaeology. It is called a book for 'young people from 14 to 18 and for adult beginners of all ages' but the packed information that it contains will make it useful also to those who are no longer beginners. The style of writing throughout is clear and simple.

The scope of the book is defined from the geographical point of view as including chiefly Britain and France, and while English Prehistory is prominent, that of Ireland and Scotland is hardly mentioned. The Mesolithic cultures of North Europe receive much attention, and France comes into the picture, chiefly because of the caves.

The book ends abruptly, as though its space was exhausted, for the whole Early Iron Age receives only 5 out of 254 pages, though for Britain, at any rate, the period was still part of her 'unwritten past'. The reader is thus left with a very inadequate picture of the state of the country 'just before history' as Mrs Plant calls her last chapter. Six printed lines seem a meagre allowance of space for the hill-top camps; for many beginners they are the most conspicuous and significant monuments of Prehistoric times within reach.

The sections of the book on physical anthropology ignore *Pithecanthropus*, and deal first with Piltdown and Swanscombe man, but omit to mention one very significant feature of those crania—their abnormal thickness, by modern standards; and though broad and long heads are recognized, it is not until the chapter on the Megalithic people that these terms are explained. Even then, the use of calipers is not mentioned, though without that instrument no one can understand the measurement of cranial length and breadth without including height. This is not an academic quibble, for young people are apt to be much interested in head-forms and racial characteristics when their nature is grasped.

The Palaeolithic section of the book is clear and interesting, and its geological background is graphically set out. Mrs Plant tells us that the Chellean peoples were flesh eaters, and 'killing their prey must have taken up most of their time'. Nourishment must have been the chief preoccupation of all food-gatherers, but why should the men of the warm Chellean epoch, ill-equipped as they were, go often to the bother and danger of killing the larger mammals, or, as is suggested, their young, except, perhaps in winter? They must often have fed on berries, roots and nuts, not to mention the young and eggs of birds, caterpillars, insects and fish. We disagree with the statement that 'living in the north the flake tool-makers were very likely stronger than the hand-axe men before whom they retreated when the warm inter-glacials set in'. Surely the less congenial climate of the north would make for a harder struggle for food, and less chance of survival against the better fed and more vital men of the—not tropical—but genial south?

The chapter on the upper Palaeolithic period has attractive pictures, but no one intimately acquainted with a non-show-cave could concur with the translation adopted

by Mrs Plant of 'home art' or 'furnishing art' for the French 'l'art mobilier'. *Mobilier* is a noun, so that while 'furniture art' might more nearly express it, surely 'chattel craft' is what is really meant, for a cave may have been a dormitory or a cemetery, but can hardly have been a home in any modern connotation, nor was it furnished. We must note that somehow the Cheddar caves are omitted from the list of upper Palaeolithic caves in England and Wales (p. 114).

The various Mesolithic cultures are admirably surveyed, though an illustration of the Lyngby axe would be welcome; but among the inventions of the New Stone Age the arts of spinning and weaving are not included, and only receive the most cursory mention near the very end of the Bronze Age, where we read that 'spindle whorls and loom weights show that weaving was practised'. Even if these arts did not reach England in Neolithic times, yet their discovery in the East and practice in Switzerland must have profoundly influenced the lives of, at any rate, the women. Similarly of pottery. Two reasons are given for its importance, one that it provided an opportunity for men and women to develop their artistic powers, and two, that it is a godsend to the prehistorian. Surely pottery was of primary importance as a domestic convenience, making it possible to drink without going to the river or pond each time, to cook in different ways, and to store food satisfactorily.

Before Mrs Plant deals with the Swiss lake-villages or the Neolithic causewayed camps there is a section on megalithic tombs with an illustration of Belas Knap, and, important as the Cotswold Megaliths are we feel that Wiltshire and many other counties will quarrel with the statement that 'besides Kit's Coty House there are many more dolmens in Gloucestershire and Wales', and we disagree with the ascription of flint-mines to chalk and limestone districts (p. 194). One of the attractions of a limestone district from an archaeological point of view is the certainty that any flint found has been imported by human agency.

The Bronze Age fills two chapters, into which a great deal of information is packed. It would have been helpful if more emphasis could have been laid on the influence of the eastern Mediterranean on certain phases of the British Bronze Age, because for many people it is a particularly suitable way of linking the unknown on to the known. We do not know why, in dealing with palstaves, we are told that the loop is peculiar to Britain, for surely the single loop was common in France, and the double loop a feature of Spanish implements.

When much is so good, it is ungracious to pick out small points for criticism, but we are sure that a second edition of this book will soon be called for in which they can be amended. The illustrations, both in the text and plates are generous; the tables very useful, and the bibliography is oddly selective. Thus, Sir Cyril Fox's *Archaeology of the Cambridge Region* is included, but not his invaluable *Personality of Britain*. Only one, and that *London and Middlesex* of the County Archaeologies series is mentioned, though everyone likes to learn about their own district, and Dr Cecil Curwen's volume on Sussex would be specially useful because of the chapter on primitive agriculture. The half-inch and inch Ordnance survey maps of England and Wales are recommended because of the megaliths marked on them, but the maps of Neolithic Wessex and of the Celtic fields on Salisbury Plain are not given, and, while we cannot commend too highly the heavy type in which the reader is told that 'for keeping your Prehistory up to date the best publication is ANTIQUITY', yet, as ANTIQUITY in no sense is limited to Prehistory, perhaps the Journal of the Prehistoric Society might march beside it.

DINA PORTWAY DOBSON



## REVIEWS

ANTIQUITIES OF THE IRISH COUNTRYSIDE. By SEAN P. O RIORDAIN, Professor of Archaeology, University College, Cork. *Cork Univ. Press*, 1942. [Historical and Archaeological Papers, no. 4]. pp. 57 and 47 illus. 5s.

This excellent and useful handbook of Irish field-archaeology 'is intended to give some answer to the questioning man in the street', and it serves its purpose admirably. It is composed on the same lines as the handbook *Field Archaeology* produced by the Ordnance Survey, but is fuller and contains illustrations. There is a classified bibliography. The outlook is modern and professional without being abstruse. The few comments we have to make are of the kind that a reviewer inserts to pad out his notice of a good book which offers few loopholes for serious criticism.

The author retains the word 'dolmen', which has been officially abandoned in this country (p. 36). Admittedly the substitute 'burial-chamber' may be inaccurate if the monument is not the chamber itself but the remains of a portal. But, although not familiar enough with Irish field-archaeology to speak authoritatively, we still doubt whether the 'pure dolmen' ever existed as a type, unassociated with a cairn or other stonework.

It is interesting to note that saucer-barrows in Ireland as in Britain belong to the Late Bronze Age and survived into the Early Iron Age (p. 41). But if by 'ring-barrow' is meant disc-barrows, these are not just saucer-barrows without mounds, but a quite distinct and earlier type of barrow. 'Pond-barrows', as Colt Hoare called them (not mentioned here), are a rare type whose date is unknown; these are sometimes called ring-barrows; they consist of a bank enclosing a flat depressed area without any mound.

There are a few air-photographs (fig. 11, opp. p. 18, would have been better printed the other way up), and there would doubtless have been many more if they had been available. There is a great and untilled field for a pioneer in this branch of research in Ireland. Good results can only be obtained by acquiring the necessary technical skill, studying the correct lighting (a low sun usually), and by an enthusiast who carries out archaeological air-reconnaissance with a camera. In this way it is certain that many more areas of early fields will be discovered, supplementing those which the author himself was the first to recognize in Ireland.

O.G.S.C.

## Books Received

EXCAVATIONS AT OLYNTHUS, part XI. NECROLYNTHIA, a study in Greek Burial Customs and Anthropology. By David M. Robinson. (The Johns Hopkins University Studies in Archaeology, no. 32). *Baltimore: The Johns Hopkins Press; London, Humphrey Milford; Oxford University Press*, 1942. pp. xxvii, 279, 71 plates, 26 figures. £4 10s.

THE LEGACY OF EGYPT. Edited by S. R. K. Glanville. pp. xx, 424, 34 plates. *Oxford: Clarendon Press*, 1942. 10s.

DIONYSIAC SARCOPHAGI IN BALTIMORE. By Karl Lehmann-Hartlebon and Erling C. Olsen. *Institute of Fine Arts, New York University and The Trustees of the Walters Art Gallery, Baltimore*, 1942. pp. 82, frontispiece and 44 figures. Price not stated.

THE WEST HIGHLANDS AND THE HEBRIDES: a geologist's guide for Amateurs. By Alfred Harker. *Cambridge University Press*, 1941. pp. xxiii, 127. 8s 6d.

BIRMINGHAM AND MIDLAND INSTITUTE. BIRMINGHAM ARCHAEOLOGICAL SOCIETY. Report of Excavations at Wroxeter (the Roman city of Viroconium) in the county



## ANTIQUITY

- of Salop, 1923-27. By Donald Atkinson. *The University Press, Oxford*, 1942. pp. xviii, 387, 73 plates (3 in pocket). 21s.
- ON THE SACRED ORIGIN OF THE GERMANIC DEATH PENALTIES. By Folke Strom. *Stockholm: Wahlstrom and Widstrand*, 1942. pp. 300. 8 kronor.
- AMARAVATI SCULPTURES IN THE MADRAS GOVERNMENT MUSEUM. Bulletin of the Madras Government Museum. By C. Sivaramamurti. *Madras*, 1942. pp. 376, 65 plates. 14 rupees 8 annas.
- POETRY AND PROPHECY. By N. K. Chadwick. *Cambridge University Press*, 1942. pp. xvi, 110. 7s 6d.
- DIWYLLIANT GWERIN CYMRU. By Iorwerth C. Peate. *Brython Press, Hugh Evans*, 358 Stanley Road, Liverpool. pp. xvi, 154. 7s 6d.
- PLACE-NAMES OF MIDDLESEX, apart from the City of London. By J. E. B. Gover, Allen Mawer and F. M. Stenton, with the collaboration of S. J. Madge. (English Place-Name Society, vol. 18). *Cambridge University Press*, 1942. 18s.
- THE ROADS OF LAUDERDALE. By R. P. Hardie. pp. xv, 106, map. *Oliver and Boyd*, 1942. pp. xxxiv, 238. 7s 6d.
- WHAT MEAN THESE STONES? The Significance of Archaeology for Biblical Studies. By Millar Burrows. *American Schools of Oriental Research*, 409 Prospect Street, New Haven, Conn., 1941. pp. xvi, 306, 58 figures, maps (end sheets). 2 dollars 50 cents.
- TWO CELTIC WAVES IN SPAIN. By P. Bosch-Gimpera. (The Sir John Rhys Memorial lecture, British Academy). *Oxford University Press*, 1941. pp. 126, maps and 6 plates. 9s 6d.
- AESCHYLUS AND ATHENS: a study in the Social Origins of Drama. By George Thomson. *London: Lawrence and Wishart*, 1941. pp. xii, 476. 21s.
- ART IN ANCIENT MEXICO. Selected and photographed from the Collection of Diego Rivera by Gilbert Médioni and Marie-Thérèse Pinto. *Oxford University Press*, 114 Fifth Avenue, New York, 1941. pp. xix, 259 illustrations. 10 dollars.